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HOW OLD IS THE WORLD?

By the Rev. H. N. HUTCHINSON, B.A., F.G.S.

GEOLOGY is not one of the exact sciences with a mathematical basis, like chemistry or electricity. Nevertheless, problems arise now and then which are capable of mathematical investigation. The problem of the earth's antiquity, or rather, that of the duration of *geological time*, which is not the same thing, is one that has attracted much attention, and has led to a long controversy between certain physicists on the one hand, and geologists on the other. According to the "nebular hypothesis" now generally accepted, our planet had cooled down from a molten and somewhat viscous state long before geological time began—that is, before a watery ocean settled down by condensation from a heated atmosphere, and left our air as it now is, mostly composed of the incombustible element nitrogen, with a little oxygen, a variable amount of aqueous vapour, and a trace of carbonic acid. How many æons passed away before this state of things was arrived at, no one can say. Such times were pre-geological. But at last an ocean formed, then, perhaps later on, dry land appeared; the wind blew and the rains fell, as they do now, and the earth reached a phase which geologists believe to have been, generally speaking, not very unlike that of the present day. The question of geological time is the question of the duration of this phase. The great series of stratified rocks (including lava flows and intrusive igneous rocks, such as "dykes") were formed during geological time; and these are the pages on which the earth has recorded her history. Naturally, therefore, the geologist endeavours to seek for some means of calculating the length of time required by Mother Earth to write her autobiography.

Now the earlier modern geologists, Hutton and his followers, who, by teaching the great principle of uniformity in geological actions, placed the science on a sound and reasonable basis, and gave it an enormous impetus, were, unfortunately, so greatly impressed with this idea that they could see no trace of a beginning or sign of an end. Sir Archibald Geikie, in his recent address as President of the British Association, assembled in Edinburgh, has thus eloquently described their state of mind: "When the curtain was then first raised that had veiled the history of the earth, and men, looking beyond the brief span within which they had supposed that history to have been transacted,* beheld the records of a long vista of ages, stretching far away into a dim, illimitable past, the prospect vividly impressed their imagination. Thus the idea arose and gained universal acceptance, that, just as no boundary could be set to the astronomer in his free range through space, so the whole of bygone eternity lay open to the requirements of the geologist. . . . This doctrine was naturally espoused with warmth by the extreme uniformitarian school, which required an unlimited duration of time for the accomplishment of such slow and quiet cycles of change as they conceived to be alone recognizable in the records of the earth's past history."

This extreme teaching, in itself a reaction against the old-fashioned previous teaching, produced another reaction, and the pendulum of opinion swung back to some extent; only slightly, but still sufficiently to raise a controversy. The physicists, led by Lord Kelvin (Sir William Thomson), began to look about for some means of checking these enormous demands. Lord Kelvin considered the question of the world's antiquity from the physical standpoint. His arguments, or rather calculations, were based on three important considerations. These we must notice; but as our object in this paper is to consider purely *geological* measures of time, and his methods can only be judged by the mathematician and astronomer, we must content ourselves with a very brief account of his conclusions. Lord Kelvin arrived at a very different conclusion, and this was derived from three distinct lines of reasoning, or rather calculation. *First*, he considered the internal heat, and rate of cooling of the earth; *secondly*, the tidal retardation of the earth's rotation; and *thirdly*, the origin and age of the sun's heat.

With regard to the earth's heat: the rate of increase of temperature downward from the surface is known, for a certain distance, by observations in mines. As many of our readers are already aware, it is about 1° F. for every 50 or 60 feet. But this rate is not maintained, and becomes less at great depths. Then with regard to the earth's present temperature—about 36° F. at the bottom of the oceans. From such available data he calculated that the earth could not have consolidated, from its former molten state, *less* than 20 millions of years ago, nor *more* than 400 millions. In the one case the underground heat would have been greater than it actually is; in the other there would have been no sensible increase in temperature downwards. He afterwards inclined towards the lower limit rather than the higher one, and said that we ought to be quite satisfied with 100 millions of years for the duration of geological time. Professor Tait would even limit the

* Viz., the 4000 years of Archbishop Ussher's Chronology, a mere "pious opinion" nowhere expressed in Scripture.

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period since the earth's consolidation to 10 or 15 millions of years.*

We pass on to the argument from the tides. It is generally admitted that the daily tidal waves must, in some degree, diminish the rate of rotation of the earth on its axis. Its action has been compared to that of a brake on a wheel. At one time, then, the rotation was more rapid; in other words, the earth's day was shorter, and has since been steadily getting longer. If we assume any antiquity for the globe greater than 100 million years, he thinks the flattening at the poles would be greater, owing to greater centrifugal force having been formerly exerted by the more rapid rotation.

Lastly, Lord Kelvin has attempted calculations based upon the radiation of heat from the sun, and also upon the amount of heat generated by the falling together of meteoric masses, such as by clashing together may have given rise to the sun. He admits, however, that his conclusions from this source are, from the nature of the case, less reliable. Still, like the other calculations, they point to a comparatively small number of millions of years, perhaps about twenty. The sun may, however, have continued to receive showers of meteorites, and thus to be replenished with heat; which would disturb these calculations. Moreover, certain chemical changes may be the means of liberating heat in the sun.

But we will not dwell on these difficulties here. It is hardly necessary to say that most geologists consider that conclusions such as these are too sweeping. Seeing what vast changes have taken place on the earth—so many thousands of feet of solid rock formed by slow deposition in water, so many new forms of life introduced at certain epochs, while others were extinguished—the geologist cannot bring himself to believe that all the changes (only fully realized by those who study the record of the rocks) could have taken place within 20 or even 100 millions of years. Some, doubtless, would demand much more time, and refuse to accept even the limit of 400 millions. No one distrusts the actual calculations; but many do seriously distrust the data (or want of data) upon which they are founded. Hence a serious difference has arisen between geologists and physicists with regard to the duration of geological time. Mathematics are an excellent mill, and will grind out beautiful results; but what you get out of this mill depends very much on what you put into it, and if you put in material based upon uncertain assumptions, you must not be surprised at getting a result tainted with similar uncertainty.

Let us quit this somewhat unsatisfactory region of speculation, and see what further light can be gained from the science of geology. It will be interesting to compare any results that may be obtained with those above mentioned, and to see whether they harmonize.

The geologist knows only two time-keeping processes; one is *rock formation* (deposition), the other *rock destruction* (denudation). A third is sometimes referred to, namely, changes in the organic world, involving the appearance, from time to time, of new species, genera, families and orders of plants and animals—changes which are comprehended under the one word "evolution." But this kind of change, which has been going on ever since the oldest (Archæan) rocks were first formed, concerns the biologist more than the geologist. The biologist, as Professor Huxley said, has no clock, and must take his time from the geological clock. In other words, when, on passing from one rock formation to another, a great change in the fossils is noticed—as, for instance, in passing from Primary rocks to Secondary or from Secondary to Tertiary, the lapse of time required to bring about such evolutionary change can only be gauged by the thicknesses of the strata in which the different fossils are found, and partly, in the two cases above quoted, by the "stratigraphical break" between the two sets of strata; that is, the amount of rock denuded during the interval between the two eras. As the Greeks used to detect "the lazy foot of time" by the slow dropping of water from a clepsydra, so the geologist measures his periods by the work of water, either as a rock destroyer or as a rock former. This is our water-clock, and our two measures of time are (1) depth of rock denuded, (2) depth of rock deposited. Now the condition of the water-clock's accuracy as a time-keeper was uniformity of action, that the drops should continue falling at the same rate; so with the geological clock. These two processes, so closely related to each other, must be supposed to have been working throughout geological time (that is, the time during which the great series of stratified rocks were being formed) with considerable uniformity. This brings us back to the "theory of uniformity" originated by the illustrious Hutton, and expanded and explained by Playfair and Lyell.

Readers of KNOWLEDGE will hardly need to be told that "denudation" is chiefly effected by "rain and rivers." The consequence of denudation in one place is rock formation in another; the one is complementary to the other. In other words, the *débris* of continents is carried by rivers into lakes, seas and estuaries, there to settle down and "sow the dust of continents to be." Now rivers depend for their supplies on *rainfall;* hence, rainfall is one of the main factors in problems about denudation. Geologists believe (from a mass of evidence in the stratified rocks which it would take too long to expound here) that the rainfall has, in past periods, been pretty much what it is *now* in various parts of the world—not necessarily in *Europe*. It *may*, however, have been somewhat greater as far back as the Archæan and Palæozoic times, when, perhaps, the earth was sensibly warmer and the sun sensibly hotter. Hence, geologists consider that they are justified in attempting to form some kind of estimate of former periods of time from the two processes above referred to. Not only is it possible thus to compare one period with another and to say which was the longest, but we venture to think that it is justifiable to attempt to calculate the limits of geological time on the basis of the rate at which strata may be formed. We want to translate feet of rock formed into years. To give a mathematical basis to geology is one of the great problems of the future. What degree of success awaits such efforts we cannot say, but certain attempts have been made to gauge denudation, and to see at what rate it goes on. With regard to deposition of strata, very little, if anything, has been done, and we cannot help thinking that important results might be obtained in this direction; but of that we shall speak presently.

Let us briefly consider the first operation, namely, the wearing away of land. The subject of atmospheric denudation has been arithmetically investigated, in order to ascertain at what rate a given continent, or portion of a continent,

* There is some uncertainty as to the amount of heat radiated by the sun. Assuming Helmholtz's theory as to the source of the sun's heat, that is that it is due to the slow contraction of the sun's bulk, and assuming that the sun has derived its heat solely in this way, we may say with some certainty that the contraction of the sun to its present size from a diameter as large as the earth's orbit would not have furnished more than 20,000,000 times as much heat as the sun now supplies in a year. But from theoretical considerations it seems probable that a gaseous mass losing heat by radiation, and contracting under its own gravity, must rise in temperature. So that it is possible that the annual loss of heat in former times may have been less than at present, even though the radiating surface was then greater.—A. C. RANYARD.

is at present being worn down by "rain and rivers." Take the great area drained by the Mississippi, which is what geographers call its "basin." The area of this basin is reckoned to be 1,147,000 square miles. It is clear that all the mud, sand, &c., brought down by this great river to the Gulf of Mexico must be derived from the rocks and soil in that area; the next step is to find out how much solid matter is brought down every year. Most extensive and accurate determinations upon this subject have been made by the United States Government. As the mean of many observations carried on continuously at different parts of the river for months together, Messrs. Humphreys and Abbot, the engineers employed to investigate the physics and hydraulics of the Mississippi, found that the average proportion of sediment contained in the water is $\frac{1}{1500}$th by weight, or $\frac{1}{2900}$th by volume. But, besides the matter held in suspension, they observed that a large amount of coarse detritus is constantly being pushed along the bottom of the river. They estimated that this moving stratum carries every year into the Gulf of Mexico about 750,000,000 cubic feet of sand, earth, and gravel. Their observations led them to conclude that the annual discharge of water by the Mississippi is 19,500,000,000,000 cubic feet, and consequently that the weight of mud annually carried into the sea by this river must reach the sum of 812,500,000,000 pounds. Then, taking the total annual contributions of solid matter, whether in suspension or moving along the bottom, they found them to equal a prism 268 feet high, with a base of one square mile.

But, besides all this, there is in every river a large amount of matter chemically dissolved. This consists chiefly of carbonate of lime, dissolved by rain-water in filtering through rocks before it reaches the river. Properly to estimate the loss sustained by the surface of a river basin, we ought to know the amount of mineral matter thus removed, as well as that referred to above; and to make sure of good results, we ought to have the total volume of water discharged, from measurements made at different seasons *and* extending over a series of years. Such data have not been fully collected from any river, though some of them have been ascertained with approximate accuracy, as in the cases of the survey of the Mississippi and the Danube. As a rule, more attention has been paid to the amount of mechanically suspended matter than to the amount in solution. We must therefore confine ourselves to the former, but it must be borne in mind that the following estimates are under-statements of the truth, because the amount of dissolved matter is left out. Some of the results obtained are as follows:—The Mississippi, with a basin of 1,147,000 square miles, discharges annually 7,459,267,200 cubic feet of solid matter; the Rhone, with a basin of 25,000 square miles, discharges 600,381,800 cubic feet of solid matter; the Danube, with a basin of 234,000 square miles, discharges 1,253,738,600 cubic feet; the Po, with a basin of 30,000 square miles, discharges 1,510,137,000 cubic feet. Now as all this solid matter comes off the surface of so much land, the area of which is known, it can easily be calculated what thickness of rock must have been removed (on an average) to produce the amount brought down to the sea, as given in cubic feet. On elevated land, where mountain streams run faster, more rock is removed than over low plains or gentle slopes, where rivers run slowly. But we only want a general average for the whole area. An illustration may serve to make this clear. Given a lump of butter, containing so many cubic inches, and a slice of bread, with area so many square inches; any schoolboy could find what the thickness of the butter would be when spread evenly over the bread. The results for the great rivers were as follows:—The Mississippi removes $\frac{1}{6000}$ foot from its area in one year, or one foot in 6000 years; the Rhone removes $\frac{1}{1528}$, or one foot in 1528 years; the Danube removes $\frac{1}{6846}$, or one foot in 6846 years; the Po removes $\frac{1}{729}$, or one foot in 729 years. Now these are very important results, and since the physics of the Mississippi have been more carefully studied than those of perhaps any other river, and as that river drains so extensive a region, embracing so many varieties of climate, rock and soil, we shall probably get the best results by taking the Mississippi rate of denudation as a fair one. Let us see, then, what that rate means. It means that the surface of its basin will be lowered 10 feet (generally) in 60,000 years; supposing the rate to continue, 100 feet in 600,000 years, and 1000 feet in 6,000,000 years. Apply this to the whole of North America, the mean height of which, according to Humboldt, is 748 feet above sea-level, and we find that this continent would be worn away in about 4½ millions of years. The same kind of calculation, based upon the rate of denudation by the Upper Ganges, has been applied to the continent of Asia, and a shorter length of time was found to be required to wear it all down to sea-level. But the Ganges rate seems to be hardly a fair one; so we will keep to the Mississippi. Such calculations are made on the assumption that no serious changes take place in the way of earth-movements, raising or depressing a continent. Upheaval would undoubtedly quicken the rate of denudation, by giving greater velocity to the rivers (on account of increased fall), and in the same way depression would check the rate of denudation. But in spite of this possible element of disturbance, the result above given is an important one. Now the amount of denudation that might thus take place over the North American continent is *a mere trifle* compared to the vast denudation which *must have taken place* in order to provide the prodigious amount of solid matter contained in the whole series of stratified rocks. Their total estimated thickness is about 100,000 feet! It is clear, then, that a much greater number of millions of years was required to lay down this great series of sedimentary rocks on ocean beds, especially when we reflect that such material had to be distributed by ocean currents over vast areas, and also that many of these rocks were built up very slowly in the deeper parts of oceans by the slow accumulation of organic remains. This applies, for example, to the carboniferous limestone, the oolites, and the chalk formation.

Evidences of great denudation abound both in Great Britain and in Europe, and in all parts of the world. Thousands and thousands of feet of solid rock have been removed, and yet such phenomena were by no means spread over the whole of geological time. We can often prove that even in the interval between two successive periods enormous denudation took place, and the mind is bewildered in endeavouring to combine with any reasonable amount of time required for such intervals the much greater periods required for the accumulation of the subsequent or overlying strata. Any student who is familiar with geological sections can call to mind numerous examples of great denudation. For instance, what a vast period of time is indicated by the upheaval and subsequent denudation of the pre-Cambrian (or Archæan) rocks before those of the Palæozoic era were deposited on their upturned edges! No attempt has been made to estimate in years this interval. Or, to take another case, it is found that in many parts of our country a great thickness of the carboniferous rocks, especially the coal measures, was denuded away *before* the advent of the Secondary or Mesozoic era. Sir Andrew Ramsey has calculated (from sections drawn to scale) that a covering of rock to the depth of one mile

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was removed from the surface of the Mendip Hills, and most of this destruction took place during the above interval. No one has yet attempted to apply a rate of denudation to this case, for the uncertain elements in such a problem are many. The Mississippi rate of one foot in 6000 years would hardly be applicable, being an average for a large area including mountains, valleys, and plains; whereas the Mendip Hills are a small hilly area. If we could find the rate at which some of our present mountainous regions are being worn down, and obtain an average therefrom, it might be justifiable to apply such an average to the case in point. But mountains are composed of hard and often crystalline rocks, and this fact would tend to counteract the more rapid erosion due to the velocity of mountain streams.

We will now endeavour to point out a method that might possibly lead to valuable results if followed, and from which an average rate of rock formation might be obtained.

Take the case of the Mississippi. What becomes of all the solid matter brought down by this river? It mostly finds its way into the Atlantic, for the Gulf of Mexico is swept by that powerful current the Gulf Stream. It would not be spread *all over* the Atlantic bed, for some may be carried up to the North Sea and Arctic Ocean; and again, there are large areas in the Atlantic where no *sedimentary* deposits are forming, but only globigerina ooze, pteropod ooze, or the red clay (believed to be volcanic and even cosmic dust). These areas are far from land, and some of them are the deepest recesses of the Atlantic. Suppose that all the *débris* from the North American continent were being washed into the Atlantic only. Now this ocean is larger than North America in actual area, but we may subtract the areas devoted to globigerina ooze or red clay. What these areas are could doubtless be estimated by Mr. Murray, of the *Challenger* expedition. We do not know how much they are; but let us suppose that when this is done, an area remains equal to the continent of North America. Then it would follow that all the rock material removed from that surface of land settles down to form new rocks *on an area equal to that of the land from which it came*. Now if, taking the Mississippi rate, one foot is removed from the former area in 6000 years, it follows that about one foot is added to the latter surface in the same time. It would really be rather more, because the new material would be soft and unhardened by pressure, while the old rocks from which it came were compressed and hardened before they came up to form a land surface. But this difference may be neglected. It will thus be seen that a result of some value is obtained, namely, just what we have been seeking—an average rate of rock formation.

The question arises—"Is this rate of rock formation over a large area of sea bed, viz., one foot in 6000 years, too rapid?" We are inclined to think that it is. It might apply to strata formed in shallow waters, but it seems too high a rate for those formed in deeper waters, and certainly is inapplicable to slow-growing deposits like globigerina ooze. However, let us see what we can make of it. The whole series of stratified rocks is generally estimated at 100,000 feet—taking all the formations and adding their thicknesses together. Here would be a measure of geological time, if only we knew the average rate at which they were built up. Suppose we apply the rate just obtained and see what it leads to. If one foot is formed in 6000 years, 100 feet will be formed in 600,000 years, and 100,000 feet in 600,000,000 years. Six hundred millions of years! This is more than Lord Kelvin's extreme limit for geological time, or the time since the earth consolidated from a molten state. And yet we have taken a rate of rock formation that appears not to err on the side of rapidity; and, moreover, this calculation makes no allowance for those great "gaps" or "breaks" in the 100,000 feet of the geological record with which the student will be familiar. Again, it makes no allowance for the necessarily slow rate at which organic deposits were formed, and formations of this kind occupy no small fraction of the whole series of rocks. For example, the great mountain limestone in one district is 4000 feet thick; the chalk in the Isle of Wight is 1000 feet thick; then there are the oolites between, to say nothing of Silurian limestones below.

It is therefore not surprising that geologists are dissatisfied with the limits laid down by Lord Kelvin and others. They demand much more time than he will allow, and we think that the calculation above given justifies such a demand. His later estimate of only 100 millions of years certainly seems too small. Professor Huxley, some years ago, endeavoured as it were to make peace between the two parties in this controversy by taking the latter limit of 100 millions and applying it to the stratified rocks. If 100,000 feet of rock were formed in 100 millions of years, then the rate of rock formation would be one foot in 996 years—say, roughly, 1000 years. Now the result we obtained above was one foot in 6000 years, so that our rate is six times slower than that which follows from Lord Kelvin's computation, and we venture to think that it would be more acceptable to geologists.

One cannot help hoping that before long some attempts will be made to observe the rate of deposition in different seas. Would not observations of the amount of sediment suspended in sea-water, taking samples from various depths, be useful? But it would be better still if someone would let down vessels (like rain gauges) on to the bed of the sea in various spots, leave them there for twenty years, and then take them up and measure the amount of solid matter contained in them. They could be attached to buoys by thin wire ropes; thus the sites would be indicated and they could be pulled up. Or again, perhaps in the future an international committee of scientific men may be formed to observe and measure the amounts of *débris* brought down to the Mediterranean by all the principal rivers flowing into it! It would take a long time, but the work could be divided up, and when done we should have a fair idea of the amount of sediment settling down in that area of sea, and so could calculate the rate of rock formation that obtains there.

BEE PARASITES.—III.

By E. A. BUTLER.

(*Continued from page* 145.)

THE parasites from which solitary bees suffer belong chiefly to two orders, the Hymenoptera and the Coleoptera. We have already seen how the stores of food they accumulate for their young are liable to be appropriated by cuckoo bees; we have now to show what persecutions are inflicted upon them by other Hymenoptera, and by some very remarkable beetles, examples of the order Coleoptera. And first as to the former. Some of the most resplendent of all British insects are the ruby-tailed flies, golden wasps, or fire-tails (Fig. 5). They constitute the family *Chrysididæ*, a carnivorous group of small extent, and not unlike small bees in shape. If they were only of larger size, they would vie with the most gorgeous productions of the tropics in splendour, but small as they are, their brilliance

and beauty never fail to elicit exclamations of delight and admiration when seen for the first time. The general type of coloration is much the same in the various species, consisting of a sparkling metallic green or blue head and thorax, and a polished ruby-red abdomen. To see them at their best, one should take up one's post on a hot summer's day by the side of a steep, bare, clayey bank, perforated with insect burrows; a specimen will soon be seen coming flashing through the sunshine and settling on the bank, where it will either rest displaying its beauty like a sparkling gem of ruby and emerald, or else run about over the surface, quivering its antennæ, and every now and then tapping the ground with them in apparent excitement.

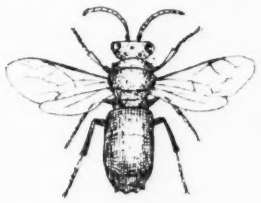

FIG. 5.—Ruby-tailed Fly (*Chrysis ignita*). Parasitic on bees. Magnified 2½ diameters.

The different species of this family frequent the burrows of various Hymenoptera, and amongst others those of certain bees, and lay their eggs in the cells in order that the larvæ hatched from them may devour the grubs for which the cells were made. By the bees they seem to be regarded with hostile feelings, whence the introduction of the egg becomes a hazardous proceeding. But they have a novel method of defence, which sometimes stands them in good stead. The last few segments of the abdomen, which taper away to a point, are, in a state of rest, telescoped up and withdrawn into the body, which thus appears to be more or less bluntly rounded at the end. When these segments are thus retracted the abdomen beneath is concave, and its junction with the thorax being a small one and extremely flexible, the whole abdomen can be bent under and folded back on the under side of the thorax, the insect thus becoming, but for its wings, almost globular. In such a condition, its hard and shining convex dorsal covering is all that is exposed, and enemies cannot make much more impression upon this than a dog can do on a hedgehog similarly folded up. When, therefore, hard pressed, the *Chrysis* is apt to adopt the policy of passive resistance, to fold itself up, and drop to the ground, lying motionless till the danger is past. The brilliant colours seem as though they might be protective in function, dazzling, and therefore warning off the foe; and it is conceivable that some insects or other enemies might be deterred in this way from attacking such bright objects. But if this be the case, the insect saves itself by inspiring a fear that is almost entirely unfounded, for though the *Chrysididæ* are bold and daring adventurers, they are not able to do much damage, having but a feeble sting, or rather no real poisonous sting at all, but only a sharp ovipositor which can give a slight prick. Moreover, their brilliance does not always secure them immunity from attack, as the following incident will show. Lepelletier de Saint Fargeau records that he once saw one of these insects, named *Hedychrum regium*, enter the burrow of a solitary bee, and being apparently satisfied with the arrangements within, it came out and, turning round, began as usual to enter backwards, in order to bring its telescopic abdomen into position for depositing eggs. Just at that moment, however, the bee returned laden with provisions, when ensued a scene very different from that witnessed by Smith in the case of the cuckoo bee related last month; on seeing the intruder, the bee at once pounced upon it, when the parasite fell back on its usual defence of doubling up into a ball, becoming thus invulnerable to the bee's sting. The latter, however, was not to be defeated in this way, and proceeded to bite off its enemy's wings, dropping the body to the ground. Thus victorious in the contest, it entered its burrow and deposited its load, and then went off on another expedition, no doubt reckoning that it had nothing further to fear from its humiliated foe. But the latter was quite equal to the occasion, and, as soon as the coast was clear, unfolded itself, crawled back to the burrow, and successfully accomplished its task.

There is also a large family of exceedingly minute Hymenoptera, called the *Chalcididæ*, which are parasites, and which are equally resplendent with the ruby-tailed flies, and sometimes even more so; they are mostly of a brilliant metallic green, or golden green, and may often be found in great numbers amongst long herbage of various kinds; but they are so minute that they would not be noticed unless carefully looked for. By sweeping amongst such herbage with a net, large numbers of specimens may often be found. Several members of this large family are parasitic upon bees, and, as they are so small, one bee's cell frequently contains a great number of their larvæ; in the cells of the *Anthophora*, referred to in our last paper, two species have been found together, and it seems probable that one of them is parasitic on the bee larva, and the other upon it, a by no means unusual arrangement in the family. The larvæ that feed upon the bee-grub are exceptional in being *external* instead of internal parasites; they cling to its body and suck out its juices, till it is completely exhausted.

Few more remarkable life-histories are to be found throughout the whole range of the Insecta than those of the beetles which constitute the family *Meloidæ*, and which, in some stages of their career, are entirely dependent upon bees for their support. So strange and unexpected, indeed, are the details of their metamorphoses, that they were for some years a great puzzle to naturalists, and the combined efforts of several most careful investigators were needed to clear up the mysteries. The commonest members of this family found in Great Britain belong to the genus *Meloë*, and are generally known as oil-beetles (Fig. 6), because they have the power of exuding from their bodies, when handled, a yellow, acrid, oily liquid. They are heavy-looking insects of a dull bluish-black colour, and wingless, although the elytra or wing-covers are developed as two flexible oval pieces lying over the base of the abdomen. The abdomen of the female becomes enormously distended by the development of the eggs, which, though individually minute, are extremely numerous, as many as four thousand having been estimated to be contained in the ovaries at one time. This enormous fecundity has relation to the life of hazard the young larva lives in its earliest days, in consequence of which no more than a very small percentage of those hatched from the eggs in all probability reach maturity. The eggs are laid in a cavity in the ground, which the female *Meloë* excavates for the purpose in early spring. The larvæ hatched from these are minute but very active yellowish creatures, with six legs and two pairs of hair-like appendages at the end of the abdomen (Fig. 7). As soon as hatched, they climb up the stems of flowers, such as buttercups and dandelions, lying in wait in the flowers for that chance which may help them on to the next stage in their life. Now, as the perfect insects are

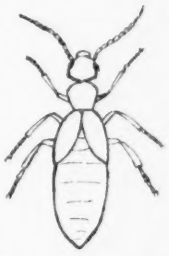

FIG. 6.—Oil-Beetle (*Meloë proscarabæus*).

vegetarians, feeding upon flowers, especially the two just mentioned, it might be imagined that the young larvæ possessed similar tastes, and had climbed into the flowers to satisfy them. But such an idea would be altogether erroneous, for in their present condition they are not vegetarians at all; their true diet consists of bees' eggs, and therefore they can make no further progress with their development until they have reached the inside of a bee's cell. Hence one would have thought that, active and enterprising as they are, they would have marched straight off in search of bees' burrows, which could not fail to be found somewhere near by; but this they show no intention of doing. Their entry into their new pasture grounds is to be made in a much more romantic fashion, for they must wait till they can secure the services of some aerial steed, on whose back they may ride in triumph to their destined quarters. Here, then, is the explanation of their presence in the flowers, for some chance bee alighting on them is to become the wished-for Pegasus. Not that any bee will do—it must be one of the *Anthophoræ* described in our last paper, or else an *Andrena*; should such an insect pay the flower a visit, the little parasite is ready to scramble on to its back while it is busy rifling the flower. One can understand with what a firm grip it would seize the bee's hairs, for it is a matter of life and death, and should one chance be missed, another may not occur for a long time. This, no doubt, explains the very peculiar shape of the parasite's feet; for, in addition to the usual pair of claws at the end of each foot, there is a central pad which looks like a third claw, and gives the foot the exceptional appearance of a three-fingered hand. These little insects are also sometimes found on the bodies of flies, but whether this is due to miscalculation on their part, or to intention, cannot be stated, nor is it known what afterwards becomes of such larvæ. There is, indeed, some doubt as to the exact course followed by the *Meloë* larva after it has gained the bee's back, but as the transformations of an allied species called *Sitaris muralis*, which occasionally occurs in this country, have been fully worked out by M. Fabre, and as those of the *Meloë* are probably almost identical with these, we will now follow the fortunes of the *Sitaris* larva.

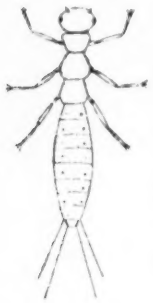

FIG. 7.—Larva of *Meloë*, much magnified. (After Newport.

The *Sitaris* is a brown beetle which is parasitic upon an *Anthophora*. Unlike those of *Meloë*, its eggs are laid at the entrance of the burrows of the bee. They are hatched in September or October, but the young larvæ, instead of at once effecting an entrance into the cells of the bees, remain where they are till the following April, taking no food all this time. The larvæ of *Meloë* seem to be equally well able to endure prolonged fasting, a very needful thing in their case, as they may have to wait long for the bee's visit which is to give them their great opportunity in life. The males of the *Anthophora* are the first to issue from the burrows in spring, and as they pass out the young parasites attach themselves to them. Soon, however, they transfer themselves to the females, and thus secure the means of entrance into the newly-made cells. When a cell has been provisioned by the careful mother with a supply of honey and pollen, an egg is laid and floats on the semi-liquid mass. Now is the chance of the *Sitaris* larva; as the egg is laid, the parasite drops upon it, apparently unnoticed by the bee, which proceeds to fasten up the cell. The parasite now has everything its own way, though care is needed, for if it were to fall off the egg it would perish in the sticky mass beneath. Sitting on its tiny raft therefore, it nibbles a hole in the egg-shell and begins to devour the contents; so small a creature is it, that this egg lasts it for a week's meals, and then it undergoes its first transformation on the empty egg-shell. No fairy's wand ever produced a more startling change than the simple process of skin-changing now effects; the active, enterprising, six-legged, slender larva becomes a fat, lethargic, almost legless grub (Fig. 8—1), whose tastes are as much revolutionized as its appearance. Animal matter not being now obtainable, a vegetable diet must perforce be substituted for it, and the grub succeeds to the inheritance of the young bee whose birth it has prevented—viz., the cell-full of semi-liquid honey; on the surface of this it floats, with its mouth buried in the mass, so that it has but to lie still and eat, and no exertions are necessary to enable it to take its fill. Its legs are therefore reduced to mere stumps, and locomotion

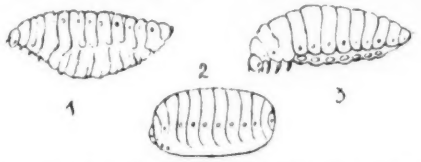

FIG. 8.—Grub of *Meloë*, showing its three stages.

becomes to it a lost art. It is so constructed that its spiracles are situated high up on its back, and therefore out of reach of the sticky sea in which it floats, and which, if it came in contact with them, would inevitably clog them up and suffocate the grub. When the honey is all consumed, a further change takes place; the grub contracts, detaching itself from its skin, which darkens and hardens, and becomes a barrel-shaped body (Fig. 8—2), within which the enclosed larva proceeds still further with its development. After a while another moult occurs, and again the form is changed, the insect appearing as a fat six-legged grub, with spiracles in the natural position (Fig. 8—3). After this it changes into an ordinary chrysalis similar to that of other beetles, and finally, after all these wonderful adventures, in the month of August the perfect beetle appears, and we are again brought back to the starting-point of the marvellous cycle of transformations. The life of an oil-beetle is very similar to this, though perhaps a trifle less complicated. From facts such as these, and the instance is by no means a solitary one, Sir John Lubbock has been led to the conclusion that, while the form of the larva of any insect is to some extent dependent upon the order to which it belongs, yet it is also greatly influenced by the external conditions to which it is exposed; in other words, that there are often changes through which an insect passes which are not to be explained by reference to the form the insect will ultimately assume, but are determined solely by the circumstances in which it then finds itself, and that therefore we may find the usual form that belongs to the larvæ of one order imitated in another, provided the circumstances of the larval life are similar, although the perfect insects produced will be totally unlike.

There yet remains for consideration a species of parasitism which affects our wild bees far more intimately than any we have yet passed in review, influencing not merely their comfort and well-being, but even their very form. The parasites in this case constitute an extraordinary family of insects which are apparently not distantly related to the *Meloidæ*; for in them we find the same transformation from an active six-legged larva to a footless grub, complicated, however, by the fact that the grubs are *internal* parasites and live, not upon the bee's food, but upon the

bee itself. The family is called *Stylopidæ*, from the chief genus *Stylops*. But we must defer the consideration of these interesting parasites till our next paper.

(To be continued.)

THE CLIMATE OF MARS.

By E. W. MAUNDER, F.R.A.S., *Assistant superintending the Solar and Spectroscopic Departments of the Royal Observatory, Greenwich.*

"THE analogy between Mars and the earth is perhaps by far the greatest in the whole solar system." Such, we have been pretty frequently reminded during the past few weeks, was the opinion of Sir W. Herschel, and it may seem very presumptuous to attempt to traverse the dictum of so great an astronomer. Still, it may be worth while to look a little closely into what we know of the Red Planet, in order to ascertain if the facts of the case really bear out this view. There are two great points of difference between Mars and the earth which strike us at once as having a very important bearing on the climates of the two planets; first, the greater distance of Mars from the sun, involving its receiving a much smaller supply of light and heat, and next, the greater length of the Martian year, rendering the effect of its seasons more pronounced.

Under the first head we find that Mars only receives, on the average, three-sevenths of the light and heat which falls upon a similar area of the earth. This would be a serious matter, even if it only meant that the mean temperature of Mars lay three-sevenths of the distance from freezing-point to the mean temperature of the earth. But as it is, we must take the absolute zero of temperature as our starting-point; that is, as is well known, 273° Centigrade, giving us from 130° to 140° below zero for our result.

This will be the mean temperature of the whole planet. But Mars must have its various zones, differing from each other as much as similar zones differ on the earth, and if we confine our attention to the equator of Mars as the hottest region, we may get a somewhat closer approximation to its condition, for it is easy to find a zone on the earth, where, owing to the oblique presentation of the surface towards the sun, the light and heat incident on any square mile is but three-sevenths of that falling on an equal area at the equator. This we find in Lat. 62°. We may therefore take the difference in mean temperature which we find between Iceland and Archangel on the one hand, and Cayenne and Singapore on the other, as affording some indication of the difference between the equatorial temperatures of the two planets.

This is, of course, to put the best possible construction on the matter, for the circulation which goes on continually in air and sea tends greatly to diminish the difference between the climates of the several terrestrial zones; so that Singapore is cooler and Iceland warmer than it should be if we considered latitude alone. This second mode of approaching the problem confirms the first to this extent at least, that it gives the mean temperature of even the hottest region of Mars as below 0° C., and consequently the mean temperature for the planet as a whole must be lower still.

Nor is this all. We know that the atmosphere of Mars is far less dense than that of our own world. This is not so obvious a difference as the two already mentioned, but it is not less certain, for it is a direct conclusion from the smaller size and mass of Mars, two points which might not at first seem to have anything to do with temperature.

For the mass of Mars is only one-ninth of that of the earth, and this implies, when its smaller diameter is taken into consideration, that the force of gravity at its surface is less than two-fifths of that which prevails here. If the strength of terrestrial attraction were diminished till it only equalled the Martian attraction, our atmosphere would at once expand upwards to more than two and a half times its present extent, and the pressure on the surface of the earth (as measured by a spring balance) would be only $5\frac{1}{2}$ pounds instead of 14 pounds to the square inch, and the aneroid barometer would read $11\frac{3}{4}$ inches. So, if we assume that there is the same quantity of air above every square inch of the surface of Mars that there is for our earth, it would extend to a greater height, and exert a smaller pressure in this proportion.

This makes the condition of "our nearest neighbour" yet worse. To find a terrestrial parallel, we must not compare Archangel or even Spitzbergen with Cayenne, but must compare the summit of a mountain more than four miles high in the Arctic regions with a place at sea-level on the equator. The result would appear to be conclusive—that the mean temperature even of the equator of Mars lies far below the freezing-point.

Of course, it might be that Mars had so extensive an atmosphere that the pressure at the surface was as great as here, or even greater. If we could imagine the pressure double that which we experience, we might suppose that such an atmosphere would go far to compensate for the diminished supply of solar heat. But that would mean a total atmosphere over the unit of surface more than five times as great as we have here; and we may feel perfectly sure that such is not the case.

For, under such circumstances, we could not perceive the surface markings of the planet. Now there is a very marked gradation in the distinctness of the surface markings of the different planets. Those of the moon are perfectly distinct; no atmospheric veil hides them from us. The markings on Mars are much less apparent, and yet are tolerably distinct. It needs but a small experience to prove that the trouble in distinguishing them, down even to the very limit of telescopic minuteness, lies far more with our own atmosphere than with that of the planet. This present opposition seems to be a hopeless one for English astronomers, but that is not in the least because Mars is modestly concealing himself behind any cloud veil; it is simply that he rises so little over our horizon that we have to observe him through a great thickness of our atmosphere. Venus, again, is a far more difficult object, and such of its markings as are permanent, if any deserve that designation, are only to be made out under specially good conditions and by trained observers. Jupiter, of course, shows us a wealth of beautiful details, but it does not require a long scrutiny to see that these are purely atmospheric, and few observers will be prepared to affirm that we have ever seen any part of its real surface. It is clear, then, that so far as these four bodies are concerned, the denser and less transparent atmosphere is always found in connection with the larger planet. Mars, on this principle, should have a less atmosphere than the earth, not a greater one.

Further, Prof. Langley and Prof. Pickering have shown us that the loss of light of a ray entering our zenith is probably just about one half, the rest being partly absorbed by our atmosphere, but mostly reflected and diffused by the minute dust particles with which it is filled. It is clear, then, that an observer on Venus would have the greatest difficulty in making out the chief features of our geography, for a large proportion of the light he received from us would come from our atmosphere,

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and of the light which fell on the actual surface only a small proportion would be reflected back, and this would again be reduced on its outward journey. It is scarcely likely that the difference in the reflective indices of land and water, especially when we remember how much of the former is covered by dark vegetation, would be sufficiently great to make itself noticeable. Of course, whenever our sky was covered by cloud the details of our geography would be effectively hidden. We may take it as pretty certain that our earth, so far from being as easy to observe as we find Mars, would be probably more like Venus; so that, instead of giving a greater amount of atmosphere above each square inch to Mars than we possess, there is little doubt but that there must be less. Indeed, if the total amount of the Martian atmosphere bears the same proportion to the mass of the planet as is the case here, then its density at the surface will be one-seventh of ours, corresponding to the state of things we should find if we could ascend in a balloon to a height of very nearly ten miles.

All these considerations point to the existence of intense cold upon Mars, cold distinctly below the freezing-point even for the equator; and yet observation does not seem to confirm this view.

Instead of the white icy glitter we should expect if Mars were one vast glacier, he sends to us a most conspicuously ruddy light. The intensity of its redness is, indeed, the most obvious thing about the planet. Then, again, it seems that its atmosphere contains a very appreciable amount of water vapour, a thing which we cannot reconcile with an extremely low temperature, for Dr. Huggins, observing the spectrum of the planet on February 14th, 1867, detected traces of some of the telluric bands due to water vapour, and though the observation is a very delicate one, and one may readily be deceived in the matter, I have repeated it myself on two occasions, and have little doubt as to its accuracy. If we accept these observations, then the white polar caps may be reasonably ascribed to ice or snow; indeed, as Proctor pointed out, even if we had never seen them, we could, after Dr. Huggins' observation, have confidently predicted their presence, and their gradual increase during the planet's winter, and diminution through its summer. Take the two facts together, and we come as nearly to a demonstration of the existence on Mars of land, water, ice, snow, and cloud as we could hope with our present resources, and sceptics must be able to produce a very strong case to overthrow it.

But how is it then that we do not find Mars completely ice-bound? For it clearly is not. In the summer of the southern hemisphere the white pole cap has been watched to shrink, until in 1877 the pole itself was actually clear, and the cap itself had a radius of only 130 miles; much as if the legend of "an open polar sea," so false for the earth, were true for Mars, and only the highland glaciers of some Martian Greenland were left unthawed under the summer sun.

So that the Martian summer would seem to be actually hotter than our own. And the winter, on this way of looking at it, is no colder. For the winter pole cap only reaches Lat. 40° or 45°, if indeed so far as that; that is to say, no lower than it does for terrestrial continents. Thus, if we refer to a beautiful series of drawings by Mr. Knobel, the President of the Royal Astronomical Society, published in Vol. 48 of the *Memoirs* of that body, we find that the Newton Sea was clear on February 11th, 1884, up to S. Lat. 50°, the Zollner Sea on February 26th to S. Lat. 50°, and the Maraldi Sea on March 8th up to S. Lat. 40°. These dates were all after February 3rd; the mean date between the autumnal equinox and winter solstice. Another series by Dawes, in Vol. 34 of the same publication, shows us Nasmyth Inlet in N. Lat. 43° on November 20th, 1864, the Delambre Sea in N. Lat. 55° on November 26th, and the Campani Sea in N. Lat. 65° on December 1st; the winter solstice having fallen on July 28th, and the spring equinox not coming on until January 4th, 1865; whilst the superb series by Mr. Green in Vol. 44 show the Nasmyth Inlet in N. Lat. 43° on September 10th, 1877, only a fortnight before the winter solstice, September 26th.

Now, although broad oceanic expanses are not covered here on our world to any low latitudes, none of these Martian markings can fully claim to be of that character. The Nasmyth Inlet is most strictly a "mediterranean" sea, and may fairly correspond to the Black Sea, Sea of Azov, and Caspian Sea. But the northern portion of the latter is ice-covered every winter; the Straits of Yenikalé are often bridged with ice, and navigation is stopped on the Sea of Azov. It would seem therefore that the winter of Mars is little if any severer than here. And we must remember, in addition, that we cannot discriminate between snow and cloud, and that Mars is free from *both* to as high a latitude as the earth is free from snow.

How are we to reconcile results so contradictory? The first circumstance to bear in mind is that the heat and light incident upon a planet are no measure of that effective upon it. Of that which falls upon the earth, probably nearly one half is reflected off again from the atmosphere alone, and a yet further amount is reflected from clouds. All this is ineffective in warming either air or soil, and if we could assume that all the heat falling on Mars was used directly by it, we should find it would be practically as well off as ourselves. And no doubt some approach is made to this condition, for although the determination of the *albedoes* of the respective planets has been much neglected, and such results as have been obtained are very rough, yet it is most plain that the albedo of Mars is very low, not differing much from that of white sandstone, whilst the albedoes of Jupiter and Venus are very high, and are best explained by supposing that the reflection takes place from a cloud shell. Zollner gives the moon as below Mars, the former being one-sixth, and the latter one-quarter. Chacornac rates Venus as five times the moon; whilst Seidel only credits Mars with an albedo of an eleventh. It is plain that we cannot put much reliance on the actual figures, but at the same time we may safely conclude that the reflection from Mars is almost as completely from the surface as it is with the moon, whilst from the earth, and yet more from Venus, it is chiefly from the atmosphere, either from cloud or from suspended dust, the latter agent being probably the more effective in the case of the earth, and the former with Venus.

It may be concluded then that the effective amount of heat received by the three planets is far more nearly equal than would at first appear. Mars is, therefore, much warmer than its distance from the sun would imply; but on the other hand its mean temperature is by no means so high as we might infer from the size of its polar caps. For we must not forget that we see the planet at the best advantage; the side it turns to us is the side it turns towards the sun; the parts of Mars with which we are best acquainted are its tropics at midsummer and at noon. The experience of our elevated plateaux and mountain tops shows how great may be the difference between the highest and mean temperatures where the air is rare, as it certainly is on Mars. There is nothing improbable in supposing that even if the mean temperature of its tropics is considerably below zero Centigrade, the day temperature

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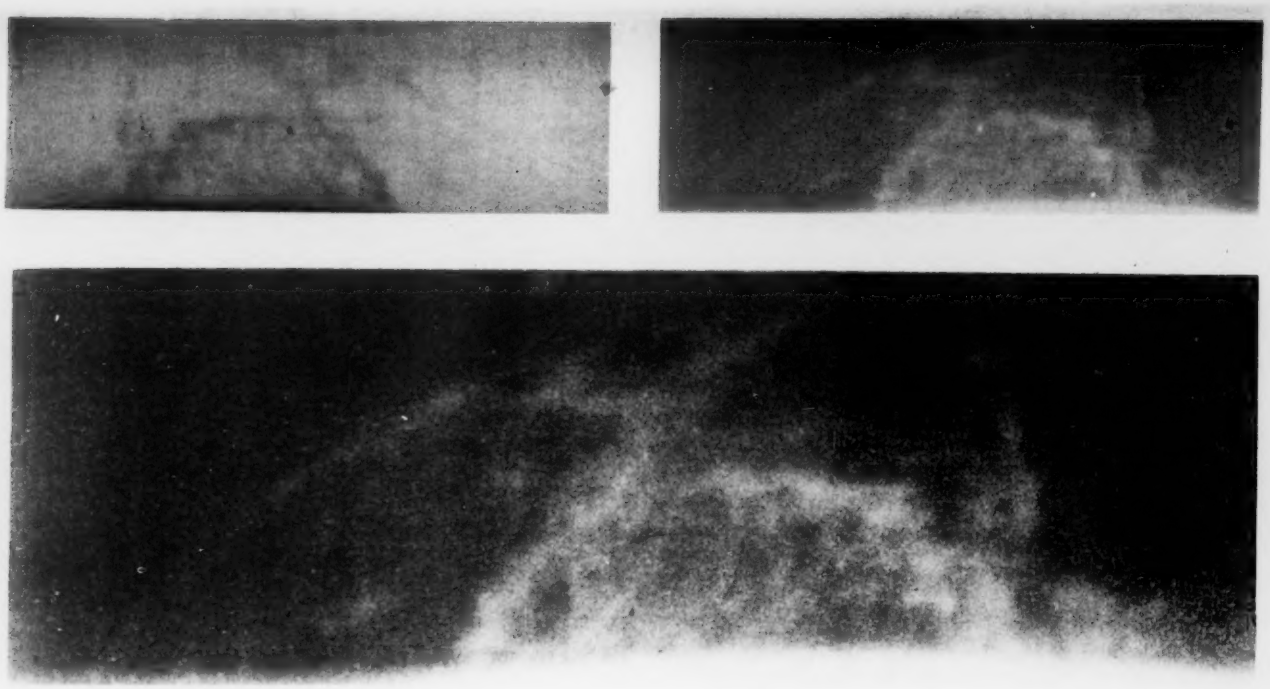
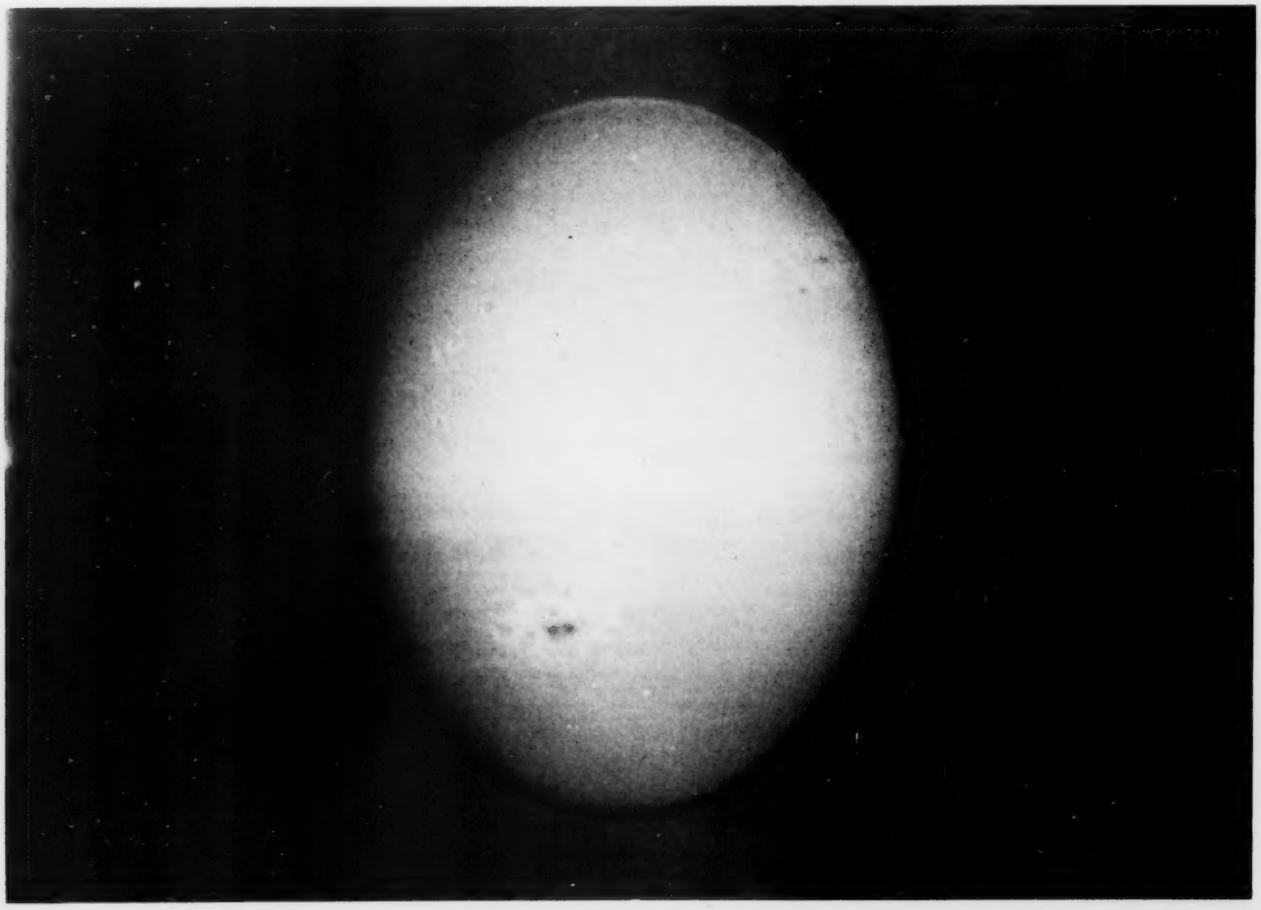


Three Prints from a Photograph of a Solar Prominence.

Taken by Prof. GEORGE E. HALE, on the 25th of May, 1892, with his spectro-heliograph, at the Kenwood Astro-Physical Observatory, Chicago. The upper right hand print is on the scale of the original negative. The upper left hand print is a positive copy reversed right and left.



Monochromatic Photograph of the Sun, showing Spots and Faculæ.

Taken by Prof. GEORGE E. HALE, with a moving slit travelling over the Sun's disc.

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may be consistently above it, and hence the ice and snow formed during the night would soon be melted during the day, and we should see only the indications of water and not of snow.

The sky of Mars by day is *almost* cloudless. We know this by observation, but we know it far more surely by inference; for observation at such a distance is apt to be deceptive. Here a most fruitful source of condensation is the lowering of temperature, consequent on the expansion which rising currents of air experience as they attain a greater height. On Mars this cause is far less effective. For that a body of air may expand to double its former volume, it must rise nearly nine miles instead of three and a half, and it will take more than six times as long to do so. Nor is this all. Its temperature would not be lowered so much by such an expansion, for less work would be done, and even an equal loss of temperature would make a less difference to the power of the air to hold water vapour. For whilst the boiling point of water would be lowered some 24° Cent. by an ascent to the height of half-density here, it would on Mars (assuming an atmosphere proportional to the mass) be lowered only 13°. It would be fairer, however, to compare the effects of an equal motion, for the nine miles rise, which would lower the boiling point on Mars by but 13°, would lower it on the earth by 85°, or six and a half times as much.

It is easy to see how it comes to pass that the spectrum of Mars affords evidence of the presence of water vapour. For evaporation would be easy and rapid, the boiling point of water being (on the assumption made before of an atmosphere proportioned to the mass) 46° Cent. instead of 100°; whilst condensation would be difficult and slow. The tendency would be during the day for the atmosphere to become as fully laden with water vapour as it would hold. At night condensation would indeed set in, but the formation of a continuous cloud canopy would probably interfere to check radiation, and would prevent the temperature falling as low as we should suppose. The traces of this night cloud canopy are probably seen in the white rim always observed on the east and west limbs, and always somewhat broader on the side emerging from darkness; for both mornings and evenings are always cloudy on Mars, but more especially the mornings. The low density of the atmosphere would prevent differences of pressure being set up at all comparable to those we know here; the feeble gravity of the planet would make the movements in response to such differences far more languid. There are no hurricanes in Mars. The clouds will be cirri, not cumuli, hoar frost will be far more common than snow, and the currents, such as they are, will not bring moisture enough to the pole in winter to cover it with snow to any great depth, so that the succeeding summer may well be able to melt it nearly all away.

If these considerations be correct, it is Venus and not Mars which bears the greatest analogy to our own planet; it is more nearly equal to the earth in size and mass, and hence I would suggest its meteorological conditions are more nearly similar. Mars with the smaller amount of heat that it receives, its thin atmosphere, and sluggish meteorology will, in spite of our knowing the configuration of its surface so well, present more differences than analogies. And what we do see is probably deceptive; the ruddy glow and apparently open seas of the part turned towards us are very likely perfectly consistent with the part which we do not see being bound in ice, or perhaps we should rather say in frost.

REMARKS BY A. C. RANYARD.

[I do not feel as certain as my friend Mr. Maunder that the atmosphere of Mars is less dense than our own; but I agree as to the strong evidence tending to show that the Martian atmosphere is more transparent than the earth's, and with Mr. Maunder's conclusion that it must be less dust-laden. This seems to follow naturally from the more languid character of the storms which Mr. Maunder has so ingeniously shown to be a natural consequence of the feeble gravity at the Martian surface. Most of the larger dust particles in the earth's atmosphere are suspended within a mile or two of the surface. During the last few years, it has been shown that the dusty state of the air is intimately connected with cloud formation. Thus on Mars we never observe white seas of cloud such as float so frequently over the earth's surface, and which must obscure its oceans and continents with a snow-white sheet, ever drifting and changing as it forms or dissolves.

The intense polarization of the sky, as seen from a mountain-top, proves that our upper air is full of very fine dust, which is possibly partly of terrestrial origin and partly derived from the *débris* of meteors which have been driven into vapour in the upper air. But it does not even follow that the number of meteors which plunge into the atmosphere of Mars correspond with the number met by the earth. The large majority of meteors which we encounter evidently belong to elliptic streams, and—whether we assume with Proctor and Sir R. Ball that such streams had a terrestrial origin or, as I think more probable, that they represent the remains of comets captured by the earth—it seems likely that the earth would encounter more of such closed streams of meteors than Mars; for, on the one theory, Mars, during its sun-like stage, must have been a feeble little centre of disruptive action as compared with the earth, and, on the other theory, Mars' power of capturing comets must be feeble compared with the earth's.

It is evident that the red colour of Mars is not due to the absorption of its atmosphere, for, as Webb remarks, its polar snows never have a ruddy tinge, even on the limb. We see no evening and sunrise tints on Mars. When it presents the gibbous form, the terminator where twilight effects should be visible seems somewhat more sharp than the limb or outer edge of the planet, which always appears rather whiter and more misty, as Mr. Maunder remarks, than the rest of the planet. On the earth, twilight lasts till the sun has sunk about 18° below the horizon, and if there were such a band of soft degrading light on Mars, it ought to be easily recognizable when Mars presents its most gibbous form. But this absence of detectable twilight is quite consistent with a very dense Martian atmosphere, if the air were very clear and free from dust.]

ON SOME RECENT ADVANCES IN THE STUDY OF SOLAR PROMINENCES AND FACULÆ.

By A. C. RANYARD.

IT may be said that till recently photography has lagged behind direct observation in the study of solar phenomena. The first great advance was made by Janssen, who, more than ten years ago, photographed details of sunspots and of the sun's surface which the eye could not recognize. Prof. C. A. Young, some years ago, obtained a few photographs of bright solar prominences, but they showed very little structure compared with the wonderful detail which can be seen with the spectroscope around the sun's limb. There is no lack of light for the study of solar structures, but the very abundance of light and heat increases the difficulties presented by the troubled ocean of atmosphere,

through which we look up at all the phenomena of the outer universe.

The bright light from the body of the sun lights up the dust in the atmosphere, and forms a luminous veil which is drawn over the whole sky and hides from us, under all ordinary circumstances, the brilliant prominences and fainter corona which surrounds the sun. At first it was only when the dust veil was rendered transparent by being plunged into the shadow of the moon that the veil was, as it were, rent for us, and for a few moments, during a total eclipse, we were permitted to look upon the wonderful structures about the sun. Then came the method of stretching out into a spectrum the white light dispersed by the dust veil, and observing upon the band of colour the unstretched coloured images of the gaseous structures behind the white veil. But, under all ordinary conditions, when the coloured images are bright enough to photograph, only a narrow slit can be looked through, showing a very narrow slice of the solar structure.

The ingenious advance made by Prof. Hale of Chicago, which has enabled him to photograph large prominences and monochromatic images of the whole disc of the sun, consists in employing a moving slit which is carried across the prominence intended to be photographed, or across the whole image of the sun, by a clepsydra, while another slit is evenly moved at a corresponding rate just in front of a photographic plate, so that the slit only allows one coloured image to fall upon the sensitive plate and screens it from the action of all other coloured images, and a photograph printed by monochromatic light is obtained.

The apparatus at first used consisted of a cylinder in a closed box, at the eye end of the observing telescope of a large diffraction spectroscope. The axis of the cylinder was parallel to the lines in the spectrum, and the cylinder could be rotated at a uniform rate by a small clepsydra driven by a supply of water mixed with spirits of wine, so as to avoid the chance of freezing in cold weather. A strip of flexible celluloid photographic film on the circumference of the cylinder was slowly moved in the plane of dispersion behind a narrow slit at the focus of the observing telescope. The diffraction grating was rotated until the κ line in the fourth order spectrum passed through the slit and fell upon the sensitive film. By changing the rate of the driving clock of the telescope, the sun's image was made to drift slowly across the first slit of the spectroscope, while the film rotated at the proper speed. In this way, if the motion of the cylinder is properly proportioned to the motion of the driving clock, a circular image of the sun can be built up. In the image of the sun shown on the lower half of our plate the two motions were not properly proportioned, and consequently the image is elliptical. Our plate is by no means a satisfactory one. The glass photograph kindly sent by Prof. Hale as a specimen of his work, shows two bands of bright faculæ running across the sun's disc in the region where spots are most frequently found.

The faculæ, shown in this photograph, all correspond to the bright monochromatic solar image κ, though no doubt they shine with other light as well, but the κ line is generally the most active photographically in the spectra of the prominences, to which the faculæ are evidently allied. Under ordinary circumstances, when examining the sun with a telescope, faculæ of much less extent are seen, and they are only clearly recognizable near to the sun's limb. The photographs made by the new method reveal the fact that they do not merely equal the spots in area but that they are far more extensive—in fact the spots sink into relative insignificance beside the faculæ which surround them.

Prof. Hale has recently succeeded in making photographs in which the faculæ, spots, chromosphere, and prominences are all shown on a single plate in their proper relative positions. This cannot be done with Prof. Hale's instrument in a single exposure, as the time required to bring out the prominences is much too long for the faculæ. A diaphragm covering the sun's image at the focus of the equatorial is therefore employed, and the slits are made to move across at the speed required for the prominences. At the end of the stroke the diaphragm is removed, and the slits are made to move back over the image at a much higher speed by adjusting the valves of the clepsydra. An image of the sun's surface is thus formed on the plate exactly within the image of the chromosphere formed during the whole exposure, and the whole operation can be completed in a minute. One great advantage of the photographic method is that sudden and short-lived eruptions, with which all solar observers are familiar, can be studied and measured at leisure.

The upper pictures on our plate represent a fine prominence photographed by Prof. Hale in May last. Much of the faint and delicate detail visible in the glass picture sent by Prof. Hale is lost in our copy, but our readers will no doubt be able to recognize several dark tree-like forms cutting out the light of the bright prominence. These, no doubt, correspond to a cooler group of prominences situated between us and the larger bright prominence, and they are seen reversed on the bright background.

We heartily congratulate Prof. Hale, and wish him still further success in the fruitful line of research which he has made for himself.

Letters.

[The Editor does not hold himself responsible for the opinions or statements of correspondents.]

THE NOVA AURIGÆ.

Extract from a letter from the Rev. T. E. ESPIN, *dated Towlaw, Darlington, 22nd August,* 1892.

. . . I had a letter from Mr. Henry Corder yesterday telling me that the Nova was again visible. I looked it up last night, and found it 9·2 mag., white or yellowish white. The spectrum has changed to a monochromatic one, all the star's light being concentrated in one intensely bright line, which is probably λ 500·0. The Nova, like the Cygnus one, seems to have become a planetary nebula. . . .—T. E. ESPIN.

[The changes observed in the spectrum of the Nova Cygni as it vanished seem to show that the nebular type of spectrum was not the last that it presented. Dr. Copeland and Lord Crawford at Dunecht noted the single greenish line of the nebular spectrum, λ 500 ±, as the only line in the spectrum of the Nova on 2nd September, 1877*. Some three years afterwards its spectrum was examined with the 15-inch refractor at Harvard College Observatory, and found to correspond with the spectrum of an ordinary star. Dr. Copeland was, in 1881, unable to see any trace of light with the spectroscope at Dunecht, which is sufficient proof that its spectrum could not then have been monochromatic. By 1885 it had decreased to a star of the 15th magnitude. Dr. J. G. Lohse, who examined it with Mr. Wigglesworth's 15½-inch refractor near Scarborough, was unable to detect any spectrum when examined with the spectroscope. It then appeared bluish in colour and nebulous. Thus the monochromatic nebular

* *Copernicus*, Vol. II., p. 108.

type of spectrum seems to have been presented only during a comparatively short stage in the development of this remarkable nebulous variable.

The Auriga Nova is evidently not diminishing uniformly in brightness. According to an article by Dr. Copeland, published in the August number of Professor Hale's *Journal of Astronomy and Astro-Physics*, p. 600, the Nova exhibited continual and irregular changes of brightness throughout the month of February. On the 20th of March it had dropped to the 9·1 magnitude, and by March 28th to the 11·9 magnitude.* Its present estimated magnitude, 9·2, would indicate a rise of more than 2½ magnitudes. These irregular changes in brightness and in spectrum favour the theory, suggested in the June number of KNOWLEDGE, that the light of the Nova is due to its passage through an irregular nebula rather than to a single collision or near approach of two stars moving in nearly opposite directions, as suggested by Dr. Huggins.—A. C. RANYARD.]

To the Editor of KNOWLEDGE.

SIR,—An active aurora of great brilliancy was visible here on the evening of Friday last from 9 till 10 p.m.

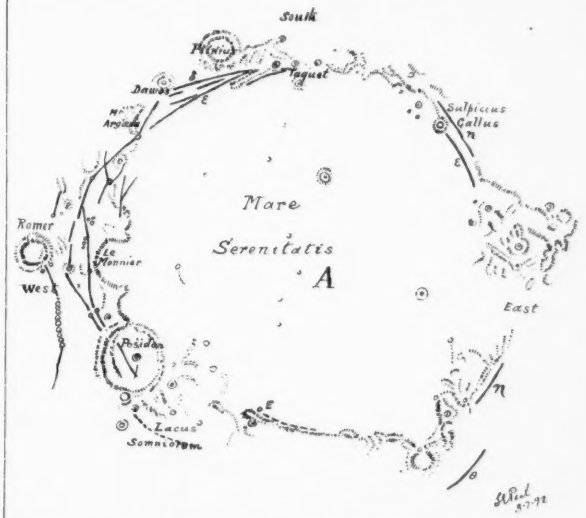
The whole of the sky, from N.W. to N.E. and from horizon to zenith, was filled with a mass of streamers. The light was sufficient for reading moderately large type. The streamers and rays were projected from the upper edge of a low arch of dark-coloured vapour resting on the northern horizon. The space occupied by the points of the streamers covered the constellations Ursa Major on the west and Cassiopœia on the east, and the intermediate region. Among the brilliant sheaf of white streamers, an occasional dark ray shot upwards from the generating arch. J. LLOYD BOZWARD.

Worcester, 17th August, 1892.

ARE THE LUNAR CLEFTS "RIVER BEDS," OR FRACTURES OF THE CRUST?

To the Editor of KNOWLEDGE.

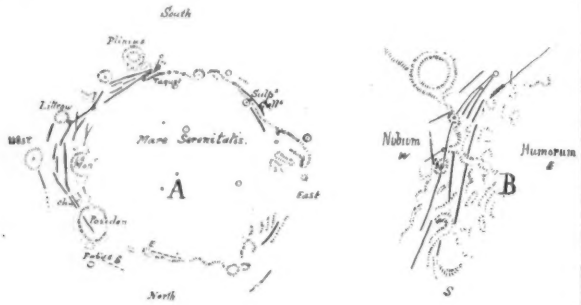
DEAR SIR,—One of the most remarkable geological discoveries of modern times is the fact that the floors of our great oceans are vast areas of slow permanent subsidence, by the steady sinking in of which, through shrinkage due to secular cooling, the persistent degradation of the continents is counteracted. Mr. J. Murray tells us that "the result of many lines of investigation seems to show that in the abysmal regions we have the most permanent areas of the earth's surface"; and M. Faye points out that "under the oceans the globe cools down more rapidly and to a greater depth than beneath the surface of the continents—at a depth of 4000 metres the ocean will still have a temperature not remote from 0° C., while at a similar depth beneath the earth's crust the temperature will be not far from 150° C." This greater relative density of the crust beneath the oceans compared to the continents has been amply proved by a long series of pendulum experiments, and is accepted as an axiom. According to Mr. Murray, "the general aspect of the abysmal regions must be that of vast undulating plains, interrupted here and there by huge volcanic cones," which rising to the surface occasionally, form the oceanic (volcanic) islands. Lying at an average depth of three miles, these areas of permanent subsidence cover $\frac{8}{16}$ths of the globe's surface, and the proofs that they have never formed part of a continental surface are very clear in several ways, one being the entire absence of stratified rock fragments (sandstones, &c.) from the ejecta of oceanic volcanoes, while so common in those existing on continents. The area of the shallow and enclosed seas is put at $\frac{3}{16}$ths of the earth's surface, that of the land being $\frac{5}{16}$ths, the mean elevation of the latter being 900 feet. Both of these contrast strongly with the abysmal plains, inasmuch as they are marked by great inequalities of level, the fluctuations being revealed in the strata. The fact for us to note specially in the foregoing is, that the slow shrinkage of our globe—due to secular cooling—is mainly taken up in and by the steady subsidence of our ocean floors, which are the coldest, densest, and heaviest portions of the earth's crust. This slow, persistent, and invariable sinking in of the sea bottoms is such a momentous feature in terrestrial surfacing that we may well keep it in view when endeavouring to solve the mystery of lunar details. The question arises, Can we trace on the moon any evidence of subsidence in the marea, or seas? To begin with, we may legitimately assume that whether the surfaces we see are composed of sand or alluvium—the sea beds laid bare—or whether they are the actual seas, solidified and floe-covered, the absence of vegetation, and of all atmospheric and pluvial erosion, would render all fractures of the crust due to mareal subsidence both more permanent and conspicuous than they would be on our earth. We may also assume beforehand that repeated (if slow) subsidence of any large circular area, such as Mare Serenitatis, would eventually result in the formation of a series of marginal tangential fissures, visible or invisible. Now, on examining the borders of this sea for evidences of subsidence, we at once find the most remarkable confirmation of the above. Beginning at the southern margin near Plinius, we find on the great incline a triple system of tangential clefts, one of which, δ, extends in a straight line from Taquet B to Dawes, while another, ε, of still greater length, runs on the north towards Mount Argæus. From thence, an almost continuous series of clefts runs tangentially around



the western border of the mare, past Littrow, Le Monnier, and Chacornac, to, and across the walled plain Posidon, on the north-west, taken up faintly in crossing Lacus Somniorum, and at the foot of the cliffs east of E, in longitude 20′ W.—the northern coast line. Again, on the eastern border at N, we have another cleft, tangentially placed, like two others, N and E, at the Sulpicius Gallus on the southern margin, thus completing the circuit of the mare,

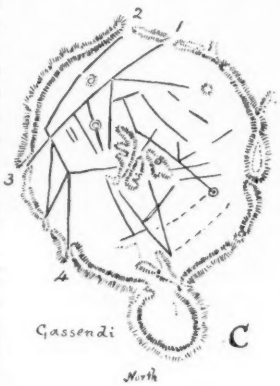
* According to Professor E. C. Pickering's measures, it had dropped to the 14·3 magnitude on April 13th. See *Astronomy and Astro-Physics*, May, 1892, p. 417.

as per diagram A. That such an arrangement of "clefts" as the above could have ever been "river beds," or *due* to river action, is simply incredible, the more so, as many of them traverse inclines athwart the drainage slopes, the "gape" of the clefts being precisely what it should appear if they were vast fissures, or marginal fractures of the crust, due to subsidence of the adjoining mare. Clearly, we are not justified in ignoring all this evidence, derived from the arrangement of the "clefts" surrounding Serenitatis. It is distinct and positive evidence against the view that they are due to river action, and equally clear that they are marginal fractures of the crust, due to the subsidence of the enclosed sea; in other words, a proof that the subsidence of sea bottoms is a lunar as well as a terrestrial phenomena. This type of arrangement is seen elsewhere, as in Mare Humorum, and may be called type A. Another type of arrangement, B, is at times seen, where two



mares are divided by what appears to be a shoal, as between Humorum and Nubium, over and along which a large group runs more or less parallel to each other. In this case (see diagram B) the group of clefts runs parallel to and over what would be the "water parting" which divides the two seas, an arrangement directly opposed to the view that they are "river beds," but precisely that which we should naturally expect if the clefts were due to subsidence of the marea on each side, east and west. A third type of arrangement, C, is where we see the clefts conspicuously forked and crossed, as in the walled plain Gassendi, at Ramsden, Triesnecker, &c. In these cases it is equally difficult to understand how such clefts could be regarded as due to river action; not only do they cross each other in all directions, in a manner quite opposed to our experience of terrestrial river beds, but in the case of Gassendi the entire system lies within the ring of the walled plain. If, however, there has been an extensive subsidence, not only of Mare Humorum to the south, but also of the mare on the north-west and west, and of the ring itself, on to and around a rigid centre, or cluster of peaks, such a complicated series of fractures will be at once intelligible. In the case of Gassendi, indeed, there is remarkably good internal evidence that this view is correct. It will be seen on reference to diagram C, drawn mainly from Neison's map of Gassendi, p. 337 of his "Moon," that the ring of this formation is gapped in two places on the southern border 1 and 2, and that each of these gaps, or passes, coincides with the extremity of a cleft on the floor inside; also that the ring is gapped in two places, 3 and 4, on the western border, each gap again coinciding with the extremity of a cleft on

the floor. This peculiarity was observed by Neison, who says at p. 342, "Some connection appears also to exist between the rill system and the peculiar passes in the walls of the formation." The connection referred to seems to be this, that the mareal subsidence and displacement to the south-west and west, which opened the crevasses or clefts on the floor, in line with 1, 2, 3 and 4, at the same time *necessarily* gapped the ring at these same places. This remarkable coincidence, therefore, of the extremities of clefts with gaps in the ring, amounts almost to a demonstration that adjoining mareal subsidence, involving, as it necessarily must, some little lateral displacement, has by the same operation both cleft the floor and gapped the ring.



Thus in the foregoing, while we have seen in A, B and C three quite distinct types of arrangement of the "clefts," they have in all cases, on examination, told the same tale—*i.e.*, that instead of being river beds, they are fractures of the crust or fissures, due to adjoining mareal subsidence. This view is indeed confirmed on a large scale by the vast series of clefts, extending from Hyginus, past Sabine, Censorinus, and Capella, as far as Goclenius, which border the Marea Vaporum, Tranquilitatis, and Fœcunditatis, and is also seen on a small scale in many places. But there are occasionally instances wherein it is not so easy to adduce "mareal subsidence" as a cause for the cleft, yet easy to show that it cannot have been a river bed. The great cleft of Sirsalis, for instance, which runs in a fairly straight line, crosses hills, valleys, craters, and mountain ranges quite impartially for some 400 miles, a series of feats which no terrestrial river is ever likely to accomplish.

But quite apart from the evidence based on the arrangement of clefts, we have in their forms alone fairly good proof that they are fractures of the crust and not river beds. As Proctor points out in his "Moon," page 176, they extend "with perfect straightness for long distances and changing direction (if at all) suddenly, thereafter continuing their course in a straight line." This notorious peculiarity is utterly opposed to all our experiences of terrestrial rivers, the sinuosities of which are well known. But the straightness and angularity is what we should naturally expect to see if clefts are fractures in a tolerably thick, brittle, and semi-rigid crust, induced by the subsidence of adjacent areas—they are not at all the features we should expect to see either in a thin crust, or one of open friable nature like sand or alluvium; in the latter we should certainly see a maze of short, irregular reticulations, instead of the great length, straightness, and angularity.

But a singularly conclusive and beautiful proof that the "clefts" are fractures, or fissures in the crust, which pass through an outer shell, down to moister and warmer substrata, is seen in their frequent structural association with small craterlets and crater cones. These are so often seen at the angles, or junctions of clefts, that they are obviously due to some deeply-seated cause for unusual dislocation of the strata. At times we see, as in Hyginus, three or four craters isolated along the cleft, in other cases they may lie close together, the well-known "crater row,"

as many as eight or ten all touching each other, as near "Romer." These little hills, having orifices on the summits, lie directly over the cleft. The so-called "river bed" does not avoid them and go round the base, hence the proof that the cleft was made first, and the craterlet, or cone, arose *on it* afterwards. There can be no doubt, therefore, that these craters and cones are piled-up formations, and hence that their invariably snow-white material has been derived from beneath the surface—through the cleft—at isolated localized passages. If these little piled cones were occasionally coloured reddish, light or dark brown, or dark grey, we might suspect they were formed of volcanic ejecta; their absolutely uniform whiteness, however, all over the globe, whether at the poles or equator, whether on mountain regions or marea, forces us to the conclusion that they are all formed of snow, exhaled as aqueous vapour through orifices along the clefts, which here and there pass down to warmer and moister substrata. Thus, while this frequent structural association of craterlets and crater rows, with clefts, is intelligible, if we look on the latter as profound fissures in the crust, it is utterly unintelligible if they are viewed as water-courses.

On the moon, as on our earth, there is yet evidently a vast store of internal heat; as this is slowly dissipated, and induces internal contraction, the lunar marea, *like our terrestrial sea bottoms*, slowly sink in, fracturing the outer crust, as we have seen, by doing so, in several different ways, as in A, B, and C; showing us also that the outer "crust" of the globe is of a somewhat brittle nature and of considerable thickness; and, lastly, that the dissipation of heat-energy is largely effected all over the surface, by the exudation of aqueous vapour through pores or passages, over and around which it piles as minute cones or "craterlets" of snow.

From the equator to the poles they are seen in countless thousands as a late or recent feature, and one of them quite naturally arising in Linné would explain the change seen therein, and solve that celebrated mystery.

Sibsagar, Assam. S. E. Peal.

[The evidence brought forward by Mr. Peal with regard to the general subsidence of the great lunar marea seems to me conclusive. The passage from Mr. Neison's book on the Moon, quoted in the June number of Knowledge (p. 115), does not refer to the rills or clefts as river beds, but only speaks of them as bearing "some resemblance" to water-courses, and as frequently commencing "at the end of a system of branched valleys leading from a highland." Mr. Neison says (p. 72), "With regard to the true nature of these rills or clefts absolutely nothing is known, whilst they are too delicate objects to allow much, if any, of the detail of their formation to be made out. It has been supposed they are cracks or fractures in the lunar surface; but their intersection and general conditions of existence seem quite inconsistent with such a supposition, more especially in their behaviour with reference to the various formations they pass through, round, or over. In many points they bear some resemblance to the dried beds of lunar water-courses or rivers, but in many features do not seem in accord with such an origin, though perhaps it presents the most feasible explanation of their nature of all; but their true nature will not be ascertained until they have been made the subject of a searching examination with a powerful telescope of the highest excellence, and thus details of the method of their construction have been obtained. Perhaps, unlike the terrestrial river beds, these rills may have arisen independently, but have served afterwards the purpose of river beds; their connection with the system of delicate valleys renders such a view somewhat probable. Thus many of these rills commence at the end of a system of branched valleys leading from a highland, whilst others can be detected winding along the bottom of extensive valley regions. At other times they appear however, entirely independent of such formations."]

THE OLDEST FISHES AND THEIR FINS.

By R. Lydekker, B.A.Cantab.

In the article on "Mail-clad Animals," published some time ago in Knowledge, it was shown how the fishes of the more ancient periods of the earth's history were frequently characterized by having their bodies protected by a coat of armour, and also how this armour has been lost by most of their modern descendants. At the same time it was mentioned that a few of these mail-clad fishes, like the gar-pike of the rivers of North America, and the many-finned bichir (*Polypterus*) (Fig. 1) of the

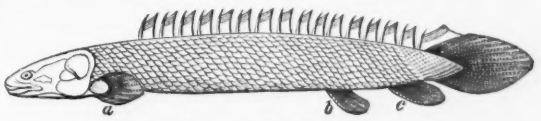


Fig. 1.—The Bichir.

Upper Nile, still linger on, as if for the purpose of showing us what their ancestors were like.

In addition, however, to their armour, and its gradual loss with the advance of time, there are many other points of view from which these ancient fishes are of more than ordinary interest, and we accordingly propose in this article to consider the curious modifications which have taken place in the structure of their fins as we ascend in the geological scale. We shall, moreover, be led to notice briefly one of the most remarkable types of fossil fish teeth found in the older secondary rocks, since it proves that one of our living fishes is the oldest kind of vertebrate now inhabiting the earth.

Before going further, we must mention that existing fishes have been divided into several main groups, distinguished from one another by structural peculiarities. One such group includes the sharks and rays, characterized by their cell-like gills and scaleless bodies. Then we have the smaller group of lung-fishes, now represented by the baramunda, of Queensland (figured in the article on "Mail-clad Animals"), and the mud-fishes of the rivers of Africa and South America, all of which can breathe either by gills or by lungs. Another group is formed by the so-called ganoid fishes (Fig. 1), many of which have the bony armour already mentioned; while the great majority of the fishes of the present day, although nearly related to these ancient ganoids, have been generally separated as a distinct group, under the title of bony fishes. That name they take from the circumstance that their skeletons are fully ossified, and do not partake of the cartilaginous nature of those of a shark or a ganoid.

Now if we look at the paired fins (or those which correspond with our own limbs) of any ordinary bony fish, such as the perch (Fig. 2), we shall see that they are formed of a number of bony rays, starting from a single point of origin, and thence spreading out in a fan-like manner. We shall also not fail to observe that the tail of such a fish has a very similar kind of structure, likewise consisting of bony rays, symmetrically arranged, and starting from a curved line where the scales suddenly stop. We may also see from the figure of the skeleton of such a fish that the backbone likewise stops suddenly where the

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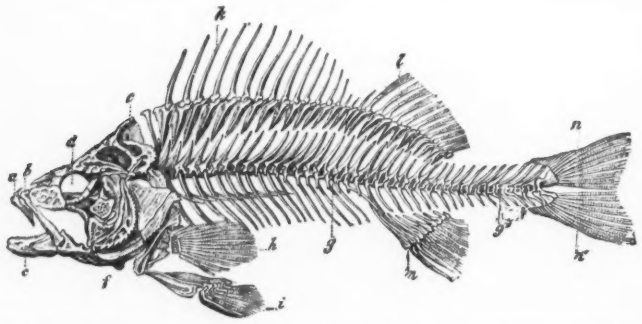


FIG. 2.—Skeleton of the Perch. *a—c*, jaws; *d*, eye; *e f*, portions of skull; *g g'*, backbone; *h*, pectoral fin; *i*, pelvic fin; *k l*, first and second back-fins; *m*, anal fin; *n n'*, tail; *k* and *i* are the paired fins.

tail begins, and that the rays of the latter start from the expanded end of the backbone itself.

The same general type of structure obtains in the paired fins of some of the later ganoids, such as the living sturgeons, as well as certain extinct forms, and it is also universally present in all the living sharks and rays. If, however, we were to go to the Natural History Museum and examine the baramunda of the rivers of Queensland, or the gar-pike (*Lepidosteus*) of those of North America, or the bichir (*Polypterus*) of the Upper Nile and the rivers of western equatorial Africa, we should find a totally different structure obtaining in the paired fins. In all these three fishes (of which, be it carefully noted, the first is a representative of the lung-fishes, while the second and third are ganoids) the first pair, or pectoral fins, as is well shown in the bichir represented in Fig. 1, are seen to have a long central lobe running for some distance up the middle of the fin, and completely covered with scales, while the rays of these fins form a kind of fringe, radiating on all sides from the central lobe, the skeleton of a fin of this type being shown in Fig. 3.

FIG. 3.—Skeleton of pectoral fin of an extinct Shark. (After Fritsch.)

From this it will be seen that such a fin consists internally of a long cartilaginous axis, composed of a number of joints (1-9), and that from one or both sides of such joints there are given off obliquely other smaller jointed rods terminating in the fine rays forming the free edges of the fins. How totally different in construction is this kind of fin from that of the perch will be manifest from our description, and a comparison of Fig. 3 with Fig. 2. Fishes with paired fins like those of the perch may be well termed fan-finned fishes; while those with fins of the type represented in Fig. 4 may be known as the fringe-finned fishes.

At the present day the only fringe-finned fishes are the baramunda and the African and American mud-fishes (which, as we have said, are the sole living representatives of the lung-fishes), together with the bichir and an allied species, and the gar-pike, which, as we have seen, belong to the ganoids. All the oldest ganoids, such as those found in the old red sandstone of Scotland, of which an example is figured in the article on "Mail-clad Animals," are, however, likewise of the fringe-finned type; and since a gradual passage from these primeval ganoids can be traced through later ganoids found in the upper palæozoic and secondary rocks to the bony fishes so characteristic of our present seas and rivers, there can be no manner of doubt but that the fringe-finned type is the most ancient one, which has gradually become modified into the modern fan-finned form.

The evidence that the fringe-finned type is the oldest does not, however, stop here; for, curiously enough, not only had the early ganoids and lung-fishes this kind of fin, but the same type likewise obtained in the primeval sharks. The fin-skeleton represented in Fig. 3 belongs indeed to a member of the same group of sharks as does the species of which the entire skeleton is shown in Fig. 4, where the rod-like axis in both pairs of fins is distinctly seen. Now we have already mentioned that modern sharks and rays have fan-like fins; and it is, therefore, clear that both in the sharks and in the compound group represented by the ganoid and the bony fishes there has been an independent transition from the fringe-finned to the fan-finned type. On the other hand, in the lung-fishes, which, as we shall see shortly, are a very ancient race, the fringe-finned structure has been preserved without alteration throughout countless ages.

We are still unacquainted with the habits of some of the living fringe-finned fishes, but at least the lung-fishes are species living partially buried in the mud, and are evidently not adapted for swimming rapidly. On the other hand, the fan-finned modern fishes, whether they be sharks or whether they be bony fishes, are generally adapted for rapid motion in the water. Any person who has watched a bowl of gold-fish will not have failed to notice the incessant and rapid motion of their film-like fins, and it is quite evident that this rapid motion could only be produced by fins of the fan-like structure. The fringe-fins are, indeed, more like clumsy paddles, capable only of comparatively slow and steady motions; such movements being sufficient for fishes protected either by the bony armour of the ganoids, or by the spines with which the early sharks (Fig. 4) were armed. The fan-like fin is therefore obviously the most specialized type of structure, and as the ganoids in their advance towards the bony fishes gradually acquired this fan-like fin, and with it, we may presume, increased speed, it was essential that their enemies the sharks should follow suit in order to be able to catch their prey. This would appear to be a sufficient reason for the attainment of the fan-like structure of fin in both these groups of fishes. It is, however, very remarkable that this structure of the fins having been independently developed in the two groups should have become so alike as it is. On the other hand, the lung-fishes, together with the gar-pike and the bichir, never having had occasion to abandon the mud-loving and sluggish habits of their palæozoic ancestors, have fortunately preserved for us intact the old fringe-finned type of swimming organs.

It is not, however, in regard to these paired fins alone that fishes show a modification from a long, jointed, axial structure, to one which stops suddenly in an expanded termination, from which arises a fan-shaped arrangement of rays, the same kind of modification, although far less generally, having also taken place among fishes in the structure of their tails.

Thus, in all the primeval fishes the backbone (as shown in Fig. 4) is continued right to the very end of the tail, where it terminates in a point. On either side of the

backbone are fringes of fin-rays, so that (as shown in Fig. 1) in scaled fishes the scaly part of the tail is continued nearly to its extremity. This type of tail is therefore exactly similar in structure to the fringe-finned type of fin, and may be similarly known as the fringe-tailed type. In some fringe-tailed fishes the fringes on either side of the tail (as in Fig. 1) are of nearly equal depth. In other instances, however, the fringe of rays on the lower side is somewhat deeper than that on the upper; and a further development of this inequality results in the partially-forked tail of the sharks, where the end of the backbone is bent upwards into the upper and longer half of the tail, the lower lobe of which is formed solely of rays. Sharks and lung-fishes have, indeed, never advanced beyond one or other of these two modifications of the fringe-tailed type. On the other hand, the compound group, including the ganoids and the bony fishes, was by no means satisfied with the primitive arrangement of matters. Starting from a fish of the fringe-tailed type like the one represented in Fig. 1, we may trace a gradual shortening of the central part of the tail-fin, accompanied by an increasing development of the rays on its lower side, until we finally reach the completely-forked tail of the perch (Fig. 2), in which, as we have seen, the backbone stops short of the fin-rays, and ends in an expanded extremity from which these rays are given off in a fan-like manner. The bony fishes have, therefore, not only succeeded in developing the fringed fins of their ancestral ganoids into those of a fan-like type, but have likewise effected a precisely similar modification in the structure of their tails. That the fan-like tail of the perch is an improvement as a steering organ upon the fringed tail of the early ganoids there can be no doubt; and it is such an organ which alone could regulate the movements of the bony fishes in the delicate manner observable in a bowl of gold-fish. To the sharks, however, whose movements largely partake of vigorous rushes, it is probable that the forked modification of the fringe-tailed type is more advantageous than would have been a tail of the fan type.

Before leaving the subject of the tails of fishes, we cannot forbear to mention that the alteration from the fringed type, with its long central axis formed by the backbone, from each joint of which springs a pair of rays, to the fan-like type, with all the rays arising together from a blunt and shortened backbone, is precisely paralleled among birds. Thus the ancient birds of the jurassic rocks, known under the name of *Archæopteryx*, had their backbone prolonged into a long tail, from each joint of which there arose a pair of feathers. Such a tail was therefore essentially a fringed one. In modern birds, however, as we all know, the backbone extends but a short distance behind the haunch-bone, and then extends into a plough-share-like bone, from which the feathers of the tail expand in a fan-like manner, very similar to the rays of the tail of a bony fish; with the exception that, whereas in fishes the fan is placed vertically, in birds it is expanded horizontally. In many groups of animals besides these we have mentioned it appears, indeed, that long tails have gone out of fashion, as being useless encumbrances. We have instances of this in the higher apes and bats, in bears, in guinea-pigs, and in the more specialized kinds of flying-dragons, or pterodactyles, described in an earlier number of this magazine.

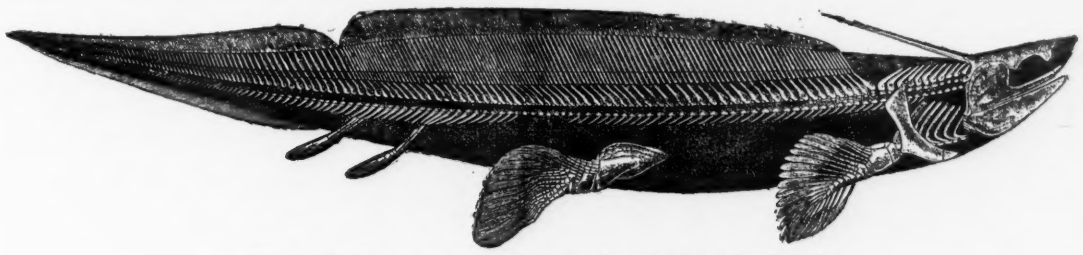


FIG. 4.—Skeleton of an extinct Shark, greatly reduced. (After Fritsch.)

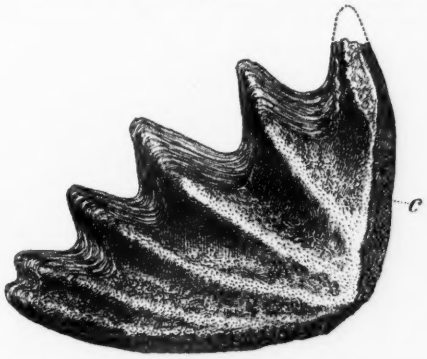


FIG. 5.—Right upper tooth of an extinct lung-fish (*Ceratodus*). *c*. Point of contact with opposite tooth. (After Teller.)

Having said this much as to the fins of the ancient fishes, we may conclude our article by giving some particulars relating to the geological history of the baramunda, which, from the structure of its fins, we have already seen reason to regard as one of the most ancient types of existing fishes. For a number of years there have been known from the triassic, or lowest secondary strata of Europe, fish-teeth of the peculiar type of the one represented in Fig. 5. The remarkable horn-like form of the ridges on these teeth suggested the name of *Ceratodus* for the otherwise unknown fish to which they pertained. Nothing more was discovered as to the nature of this problematical fish, and it was even doubtful in what position these teeth were placed in the mouth, or how many of them there were in each jaw. Thus matters stood till some twenty years ago, when naturalists were startled by hearing that a large fish had been discovered living in the rivers of Queensland, which had teeth like these problematical fossils. This fish was no other than the baramunda, which, as we have seen, is one of the few living species still retaining the ancient fringed fins. It was found that the baramunda had one tooth on either side of each jaw, placed in the same position as the figured example; and it was naturally considered that the living fish belonged to the same genus as the *Ceratodus* of the trias. Here, then, we are confronted by the remarkable circumstance that a kind of fish first made known to

us by fossil teeth from the very lowest secondary strata of Europe, was actually represented by one apparently belonging to the same genus in the rivers of Australia. It is true, indeed, that a recent discovery has shown us that the living baramunda differs slightly in the structure of its skull from the fossil *Ceratodus*, and that the teeth of the opposite sides of the jaws were not in actual contact with one another, as were those of the latter. Although these points of difference are considered sufficient to warrant us in regarding the baramunda as not actually belonging to the same genus as the fossil teeth, yet this does not detract from the extreme interest of this fish as being by far the oldest type of back-boned animal now living. This type of fish is, indeed, thus proved to have endured throughout the whole of the immense period during which the entire series of secondary and tertiary rocks were deposited; and when we reflect that the secondary rocks include those enormous accumulations of strata known as the trias, lias, oolites, greensands, and chalk, while the tertiary comprises the threefold divisions termed eocene, miocene, and pliocene, we can scarcely fail to be almost lost in wonder at the prodigious length of time during which baramundas have existed, with but comparatively slight structural modification. The fossil baramundas occur in the secondary rocks of Europe, Africa, India, and North America, and it is an interesting subject of speculation why the group should have totally disappeared from all those regions, to find a last home in far Australia.

The existing baramunda lives mainly or entirely on leaves, and we may therefore conclude that the fossil teeth likewise pertained to fishes which subsisted on somewhat similar nutriment. If, however, we were to infer that all the teeth of fossil fishes which have a ridged or flattened grinding surface belonged to herbivorous types, we should be sadly in error, since many of them, approximating more or less markedly to the *Ceratodus* type, really indicate fishes allied to the well-known Port Jackson shark, in which the mouth is covered with a complete pavement of flattened teeth adapted for crushing shell-fish and other hard animal substances. In all such investigations, the truth can, indeed, only be found out by careful induction, and by availing ourselves of every scrap of information left to us among the relics of former epochs.

RADIANT MATTER.

By A. JAMESON.

AN article on "Radiometry," that appeared in a recent issue of KNOWLEDGE, shortly described how some of the energy of ether waves, striking an absorbent surface, and being transformed into heat—that is, into disturbance of the molecules of matter—may give rise to what is known as Crookes' pressure, and to mechanical motion. It was explained that light mills, as ordinarily made, will only operate when surrounded by a gas in "the fourth condition," *i.e.*, where the free paths of the molecules were long compared with the diameter of the light mill. It is now proposed to relate briefly some of the other experimental facts through which the discovery of this condition of matter has so much improved our knowledge of the actual condition in which gaseous matter exists—that is, of the way in which the molecules of gases are continually moving.

It is perhaps unnecessary to preface the remark that this nominal distinction between gaseous and ultra-gaseous matter is, although a useful, a purely arbitrary one, and that the two conditions are really in the strictest sense of the word continuous; for the accepted definition of a gas is that it is matter, any portion of which is capable of expanding indefinitely, so as to exert a pressure upon the walls of any containing vessel, however large. Not that even this broad definition is altogether unexceptional. It might be said, for instance, to be inapplicable in the case of those gaseous masses that were described in Mr. Ranyard's article last July, and the extreme limits of which could probably be calculated in the same way and with the same degree of precision as that of the earth's atmosphere was calculated in KNOWLEDGE for November last. But this point is only mentioned incidentally; we are not much concerned with it at present. Gas molecules at a given temperature, or that are moving relatively to one another at a given average speed, are, one might suppose, unaffected individually by the pressure of the gas of which they are constituents. But there are slight exceptions to be taken to this statement also. We have, for instance, the mutual actions (attractions, and perhaps some additional inter-molecular action of adjacent molecules) making themselves felt in proportion to the square of the density, and constituting the well-known deviation from Boyle's law.* Another distinction that will be mentioned is of greater interest in connection with the subject of high exhaustion. It must be premised that molecules themselves are elastic bodies whose constituent parts are capable of oscillatory motions (intra-molecular vibrations). These relative motions of the parts of individual molecules are excited by transverse ether waves, and by molecular impacts. The average velocity of translation of the molecules of a gas represents its sensible heat; while in addition to this we have the energy concerned in the pulsations of individual molecules or chemical atoms. The intra-molecular vibrations, excited as has been described, are capable in their turn of causing locomotion of the molecule, by virtue of the reaction that takes place when this pulsating body comes in contact with any resisting body. They also give rise to radiations of transverse waves in the ether, and in fact Prevost's law of exchanges of radiant heat holds as perfectly between the molecules of a gas as in any other circumstance. Each molecule is continually throwing off ether waves at the expense of its intra-molecular energy, and is continually deriving reinforcement of that energy from the ethereal radiations of neighbouring molecules, or of surrounding substances. Now in the case, for example, of a gas undergoing the process of cooling, the oscillations of the chemical atoms die away to some extent between successive encounters, owing to the radiation of their energy. Just as a vibrating bell throws off aerial waves, so a vibrating molecule generates waves of a somewhat different character in the medium with which it is surrounded; and if the material vibrations excited by striking the elastic body are renewed less frequently, the result, similar in each case, is a diminution of the average intensity of those oscillations. Hence very highly rarefied gases, in which the interval of time between successive collisions is considerable,† in which, that is to say, the mean free path is greatly extended, must cool more slowly by radiation than would be the case were they in a condition of greater density. Such distinctions as these between the gaseous and ultra-gaseous states serve, however, to accen-

* Boyle's law, as the reader is doubtless aware, asserts that the pressure and volume of a gas are inversely proportional to one another.

† This interval in hydrogen at the ordinary temperature and pressure is, according to Professor Tait, only one 17,700,000,000th part of a second, that is to say, each particle of hydrogen has the direction of its motion changed on an average 17,700,000,000 times per second.

tuate the fact that for all essential purposes the slight differences between them are simply of degree and not of kind.

We have adopted provisionally, and merely for the sake of simplicity, the view that inter-molecular motions (the relative motions of molecules as wholes) take place in perfectly straight lines. This is certainly a near approximation to the truth. It is therefore reasonable to expect that in gases of ordinary density—in the open air for instance—effects identical with those described under "Radiometry" in the July issue of KNOWLEDGE should be obtainable; all that is necessary being that the hot and cold surfaces should be brought sufficiently closely into proximity. This has been accomplished in several ways: notably, by means of an "otheoscope" specially constructed with this object by Prof. Crookes. Another example, familiar to everyone, of molecular pressure exhibited in gas not much removed from the normal density is furnished by the so-called calorific paradox or spheroidal state. When a globule of water is placed on a red-hot stove-plate, evaporation gives rise to such a shower of radiant molecules, raining down on the hot metal from the lower surface of the liquid spheroid, that a mechanical reaction is produced equal to sustaining the weight of the drop, so that this rolls on, without touching, the heated surface. Everyone must have noticed the rapid tremulous agitation of a drop of water undergoing slow evaporation in this way; and it may not be out of place to suggest, for illustrative purposes, that these oscillations exemplify very crudely what is meant by intra-molecular vibration; only, of course, just as a molecule is very much smaller, so its vibrations are very much more rapid than anything within the cognizance of our senses.

Now to return to the consideration of gases so conditioned that the peculiarities of their inter-molecular motions may be most directly and readily shown. The description of only a few of the experiments by which Prof. Crookes has so thoroughly explored these matters, will give extension and confirmation to such explanations as have already been given. One of the first questions that presents itself is, Does *specular* reflection take place at the surface of a body from which a molecule rebounds, and is the angle of rebounding equal to the angle of incidence? The answer to this question is in the negative; for if specular reflection occurred, the only effect that could be produced by the impact of molecules would be a pressure *normal* to the surface struck. The oblique component, if any, of the molecular pressure, acting in the line of incidence, would be exactly neutralized by the oblique reaction in the line of reflection. But it is well known that the oblique component of molecular pressure is not compensated in this way. Fig. 1 shows a form of radiometer by which this fact is very prettily demonstrated. The skewed vanes are, as usual, coated with lampblack, but do not rotate. They are permanently fixed to the upright in the position shown. Above them is a movable disc of thin, smooth mica, supported in the centre upon a needle point. The upper surface of the disc is painted in sectors with pigments representing the different colours of the spectrum duly proportioned; and it is made to rotate with such rapidity by the oblique streams of molecules from the skewed lampblacked vanes, that the colours blend with one another to a neutral grey. Although it is true that some highly polished surfaces are moved by the oblique component of molecular pressure in a way inconsistent with simple reflection, there is of course considerable loss of power in such an application of the molecular pressure to the production of molar motion. Thus with the ordinary form of radiometer one is not surprised to learn that much more energetic effects are obtained when a narrow hoop of metal, transversely stepped or corrugated and painted with lampblack, is placed, for a reacting surface, horizontally within the glass globe. Under this arrangement the energy of the projected molecules is more perfectly absorbed at the moment of impact; and the "cooled," *i.e.* retarded, molecules may even be returned directly back upon the vane or fly. Indeed, a single molecule may thus be bandied very many times in succession between the vane and the reacting surface; and Prof. Crookes has pointed out that with a sufficient degree of rarefaction differences of molecular pressure may exist between different parts of the apparatus for twenty minutes or longer.

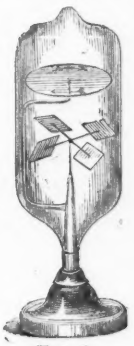


FIG. 1.

It has been asserted that the molecular wind from a heated surface blows very nearly normally to the surface; but the fact that radiant matter is also thrown off in oblique directions has been shown by the following experiment. A single "vane" resembling the half of a cone of perhaps one inch in height was made (hollow for the sake of lightness) out of thin sheet mica, and it was fixed on the end of an arm, centrally pivoted, in the same way as a magnetic compass needle. The opposite extremity of the arm carried a small metal counterpoise; and the whole was mounted in a radiometer bulb, exhausted of course, and having within it, below the pivoted arm, a large horizontal disc of lampblacked material. The semicircular base of the mica vane was horizontal, and therefore parallel with the blackened or radiant surface. The triangular (flat) surface of the vane was normal to the blackened disc, and its apex was uppermost. Hence molecules thrown off vertically from the blackened surface would simply exert an upward pressure on the semicircular base of the vane. They could not act in any way upon either the straight or the curved side thereof. Molecules projected obliquely might, on the other hand, impinge on the vertical plane side, or, *but to a less extent*, on the sloping curved side of the vane. For the same degree of obliquity of the molecular showers, the angle of incidence would be great on the vertical side and small on the sloping side of the vane. Hence it is clear, upon the whole, that oblique molecular currents would be deflected to the greatest extent by the vertical plane surface; and that their existence involves the manifestation at this surface of an excess of molecular pressure. As a matter of fact, the pressure on the plain side of the vane is enough to set the arm in rotation.

Radiometers have been made in which one of the horizontal arms to which the vanes are attached is a magnetic needle. Of course, the rotation of the vanes in such an instrument can be stopped by simply bringing a powerful bar magnet into the neighbourhood of the bulb. If this is done while the instrument floats, like a hydrometer, in a glass jar filled with water, the showers of molecules from the stationary vanes will set the glass envelope itself into rotation. A paper index, of say one foot in length, may be attached to the upper surface of the glass globe and will make this revolution more evident. Such an arrangement, in which the motion produced by light is not only visible but tangible, makes an exceedingly attractive scientific toy. Prof. Crookes suggests also that the *rotation* of the magnetic needle within such an instrument as this might be made to induce electric currents in wires placed outside the bulb, thereby operating self-

XUM

recording devices that would be most useful in meteorological work.

The efficiency of the light in different parts of the solar spectrum for producing motion at the lampblacked surface in a radiometer has been found to be as follows: Assuming that of the ultra red waves to be represented by 100 the extreme red gives 85 per cent. of this standard repulsion; red, 73; orange, 66; yellow, 57; green, 41; blue, 22: indigo, 8½; violet, 6; and ultra violet 5 per cent.

It has been suggested that a kind of mechanical process of photography might be based upon the different repulsive powers of different kinds of light. A still more interesting adaptation of the phenomena of radiant matter to purposes connected with photography, and one that has been practically carried out by Prof. Crookes, is in the study of those thermal changes which accompany photographic action at a surface. Suppose a miniature Daguerreotype plate delicately suspended in a "radiant matter bulb," the exhaustion and sealing of the bulb having been carried out in non-actinic light. If, when the apparatus is at rest and in thermal equilibrium in a dark room, a beam of actinic light is directed upon the sensitive surface, of course a certain amount of repulsion, due to molecular pressure, will take place; but such repulsion is a result of the conversion of light into heat. And hence, for so long as a great part of the incident radiation is engaged in performing the work of chemical separation, the repulsion due to that radiation must be considerably below the normal. Space will not permit, or rather it would be inappropriate to the subject of these articles, to introduce a discussion of the results of these experiments, and of others equally interesting, bearing on the absorption of the different coloured powders used as coating upon radiometric vanes. Of course, the wave-length of the radiant heat or light that is employed is an important factor that has to be kept in view in the interpretation of all such experiments.

When radiation of very low frequency is employed, it may be so largely absorbed by the glass bulb that this, in consequence of the increase of its temperature, becomes itself a generating surface for molecular wind. Effects of this kind, which may be produced also by placing a warm finger on the bulb, give rise to movements of the vanes that are at first sight very puzzling. Again, a thin platinum wire, mounted within the exhausted globe (see Fig. 2), may be so heated by being made part of a galvanic circuit as to generate very powerful currents of radiant matter, and produce mechanical effects of proportionate intensity on a "turbine wheel" radiometer. The skewed vanes in this little heat engine are usually made of sheet mica; but, of course, the nature of the material is unimportant, so long as rigid and light, for the turbine wheel simply plays the same part in relation to radiant matter that the paddles of a water-wheel, or the sails of a windmill, do to currents of water or air.

Fig. 2.

THE FACE OF THE SKY FOR SEPTEMBER.

By Herbert Sadler, F.R.A.S.

THE solar disc shows little or no diminution in the frequency of groups of spots and faculæ. The following are conveniently observable minima of Algol: September 1st, 9h. 49m. P.M.; September 4th, 6h. 38m. P.M.; September 24th, 8h. 20m. P.M.

Mercury is a morning star throughout September, and is well situated for observation during the second and third weeks of the month. On the 8th he rises at 3h. 49m. A.M., or 1h. 36m. before the Sun, with a northern declination of 11½° and an apparent diameter of 7½″, about $\frac{34}{100}$ths of the disc being illuminated. On the 13th he rises at 3h. 51m. A.M., or 1h. 42m. before the Sun, with a northern declination of 10° 46′ and an apparent diameter of 6½″, about $\frac{55}{100}$ths of the disc being illuminated. On the 16th he rises at 4h. 0m. A.M., or 1h. 39m. before the Sun, with a northern declination of 9° 36′ and an apparent diameter of 6·0″, $\frac{7}{10}$ths of the disc being illuminated. On the 23rd he rises at 4h. 38m. A.M., or 1h. 12m. before the Sun, with a northern declination of 5° 22′ and an apparent diameter of 5½″, about $\frac{9}{10}$ths of the disc being illuminated. After this he approaches the Sun too closely to be conveniently observed. He is at his greatest western elongation (18°) on the 11th, and his greatest brightness about the 17th. While visible, Mercury describes a direct path through Leo, being near Regulus on the 7th.

Venus is still a very conspicuous object in the morning sky, but both her brightness and diameter decrease considerably during the month. She rises on the 1st at 1h. 24m. A.M., with a northern declination of 17° 23′ and an apparent diameter of 28¾″, $\frac{40}{100}$ths of her disc being then illuminated, and her brightness being about the same as in the middle of May. On the 30th she rises at 1h. 44m. A.M., with a northern declination of 13° 6′ and an apparent diameter of 21″, $\frac{57}{100}$ths of the disc being illuminated, and her brightness having diminished to what it was at the beginning of April. She is at her greatest western elongation (46°) on the 19th. At 3h. 50m. A.M. on the 22nd a 7½ magnitude star will be 1′ south of the planet. During the month Venus describes a direct path from the boundaries of Gemini through Cancer into Leo.

Mars is an evening star, but is still wretchedly situated for observation in these latitudes. On the 1st he rises at 6h. 10m. P.M. with a southern declination of 21¼° and an apparent diameter of 22¼″, the defect of illumination on the following limb becoming now very perceptible. On the 30th the planet sets at 0h. 33m. A.M., with a southern declination of 21° 11′ and an apparent diameter of 17·0″, his brightness towards the end of the month being less than half of what it was at opposition. During the month he describes a direct path in Capricornus. The minor planet Pallas will be in opposition on September 20th, on which evening she souths at midnight with a southern declination of 6° 8′. The present opposition is not a very favourable one, the stellar magnitude of the planet being only 7½. During the month Pallas describes a retrograde path in Cetus.

Jupiter is now becoming a magnificent object in the evening sky, being actually visible to the naked eye in sunlight at the end of the month, the coming opposition being a very favourable one. He rises on the 1st at 8h. 1m. P.M., with a northern declination of 8° 1′ and an apparent equatorial diameter of 46½″. On the 30th he rises at 6h. 3m. P.M., with a northern declination of 6° 54′ and an apparent equatorial diameter of 49″. During the month he describes a retrograde path in Pisces. The following phenomena of the satellites occur before midnight, while Jupiter is more than 8° above and the Sun 8° below the horizon. A transit ingress of the second satellite at 9h. 49m. P.M., and a transit egress of its shadow at 10h. 24m. P.M. on the 1st. An eclipse disappearance of the first satellite at 10h. 5m. 26s. on the 2nd. A transit egress of the shadow of the first satellite at 9h. 36m. P.M., and of the satellite itself at 10h. 30m. P.M. on the 3rd. An eclipse reappearance of the third satellite at 9h. 49m. 48s., and an occultation dis-

appearance of the same satellite at 11h. 23m. P.M. on the 6th. A transit ingress of the shadow of the second satellite at 10h. 27m. P.M. on the 8th. An eclipse disappearance of the first satellite at midnight on the 9th. A transit ingress of the shadow of the first satellite at 9h. 16m. P.M.; an occultation reappearance of the second satellite at 9h. 25m. P.M.; a transit ingress of the first satellite at 10h. 4m. P.M.; and a transit egress of the shadow of the same satellite at 11h. 30m. P.M. on the 10th. An occultation reappearance of the first satellite at 9h. 26m. P.M. on the 11th. An eclipse disappearance of the third satellite at 11h. 38m. 13s. P.M. on the 13th. An eclipse disappearance of the second satellite at 7h. 59m. 53s. P.M.; a transit ingress of the shadow of the first satellite at 11h. 10m. P.M.; an occultation reappearance of the second satellite at 11h. 41m. P.M.; and a transit ingress of the first satellite at 11h. 49m. P.M. on the 17th. An eclipse disappearance of the first satellite at 8h. 23m. 20s. on the 18th. A transit egress of the first satellite at 8h. 27m. P.M. on the 19th. A transit ingress of the third satellite at 7h. 58m.; a transit egress of the shadow at 8h. 8m. P.M.; a transit egress of the satellite at 9h. 42m. P.M.; and an eclipse disappearance of the second satellite at 10h. 35m. 1s. on the 24th. This transit should be carefully watched. An eclipse disappearance of the first satellite at 10h. 18m. 11s. on the 25th. A transit ingress of the first satellite at 7h. 33m.; a transit egress of the shadow of the second satellite at 7h. 35m. P.M.; a transit ingress of the first satellite at 7h. 59m. P.M.; a transit egress of the second satellite at 8h. 20m. P.M.; a transit egress of the shadow of the first satellite at 9h. 46m. P.M.; and a transit egress of the shadow itself at 10h. 10m. P.M. on the 26th. An occultation reappearance of the first satellite at 7h. 21m. P.M. on the 27th.

Both Saturn and Uranus are invisible, and as Neptune does not come into opposition till December we defer an ephemeris of him till next month.

There are no very well marked showers of shooting stars in September.

The Moon is full at 9h. 7½m. P.M. on the 6th; enters her last quarter at 0h. 50m. P.M. on the 13th; is new at 1h. 16m. A.M. on the 21st; and enters her first quarter at 6h. 19m. A.M. on the 29th. She is in perigree at 11·0h. P.M. on the 8th (distance from the earth 225,550 miles), and is in apogee at 6·0h. P.M. on the 24th (distance from the earth 252,140 miles). The greatest western libration takes place at 9h. 15m. A.M. on the 3rd, and the greatest eastern at 5h. 10m. A.M. on the 16th.

Chess Column.

By C. D. LOCOCK, B.A.Oxon.

ALL COMMUNICATIONS for this column should be addressed to the "CHESS EDITOR, *Knowledge Office*," and posted before the 10th of each month.

Solution of August Problem (by D. R.)

1. R to R6, and mates next move.

CORRECT SOLUTIONS received from Alpha, H. S. Brandreth, W. Pennett, G. Burt, and A. R.

G. Burt.—Some of your variations, however, are incorrect. There are four different mates for the moves of the Black Bishop. (The Queen cannot mate.) After 1. . . . P to Q3, 2. Kt to B4 is the mate.

Alpha.—Generally speaking, the more variations the easier the problem; for some of the variations have to be ready-made. Duals result from Kt to Rsq or R5.

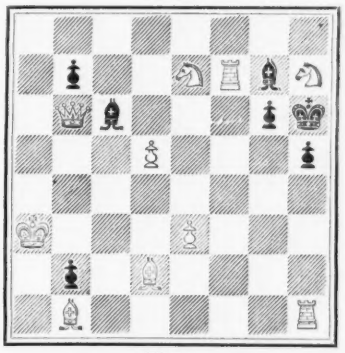
G. K. Ansell.—Thanks for the problem, which has been returned to you for revision.

PROBLEM.

By C. D. P. HAMILTON.

(2nd prize in *Illustrated American* Tourney.)

BLACK.



WHITE.

White to play, and mate in two moves.

The following fine game was played in the fourth round of the Dresden Tournament.

"RUY LOPEZ."

WHITE (Mason).	BLACK (Scheve).
1. P to K4	1. P to K4
2. Kt to KB3	2. Kt to KQ3
3. B to Kt5	3. P to QR3
4. B to R4	4. Kt to B3
5. P to Q3	5. P to Q3
6. P to B3	6. B to K2 (*a*)
7. QKt to Q2	7. Castles
8. Kt to Bsq	8. P to QKt4
9. B to B2 !	9. P to Q4
10. Q to K2 (*b*)	10. R to Ksq
11. Kt to Kt3	11. P to Q5 (*c*)
12. Castles	12. P to R3
13. Kt to Ksq	13. P to Kt5
14. B to R4 (*d*)	14. B to Q2
15. P to QB4	15. B to KBsq
16. P to B4	16. Kt to Ktsq (*e*)
17. P takes P !	17. R takes P (*f*)
18. B takes B (*g*)	18. QKt takes B
19. Kt to B3	19. R to Ksq
20. K to Rsq (*h*)	20. Kt to B4
21. Kt to B5	21. Kt to K3
22. B to Q2	22. P to QR4
23. R to KKtsq !	23. P to R5 (*i*)
24. P to KKt4	24. R to R4 (*j*)
25. QR to KBsq (*k*)	25. P to R6
26. P to Kt3	26. K to R2
27. P to Kt5	27. R takes Kt (*l*)
28. P takes R	28. Kt to B5
29. Q to Qsq	29. KKt to R4
30. B takes Kt (*m*)	30. Kt takes B
31. R to Kt4	31. B to Q3 (*n*)
32. P takes P	32. P takes P
33. P to B5 (*o*)	33. Kt to Q4 !
34. P takes B	34. Kt to K6
35. P takes P !	35. Q to Rsq (*p*)

XUM

36. Q to Bsq	36. Kt takes KR
37. Q to KB4	37. Kt to K6
38. R to KKtsq	38. Q to B3 (*q*)
39. P to B6 !	39. Kt to Kt5 (*r*)
40. R takes Kt	40. Resigns.

NOTES.

(*a*) A very questionable defence, adopted by Lasker in one of his games with Blackburne. After castling the Bishop becomes a fixture, hampering the movements of Queen and Queen's Knight. P to KKt3 is the correct defence.

(*b*) Preventing the exchange of Queens: *vide* Steinitz *v.* Tschigorin.

(*c*) Here, as usual, a weak move, for the Pawns are liable to ultimate disintegration by P to KB4. Perhaps his best course was 11. . . P×P, 12. P×P, B to Q3. His next move is probably made with the intention of answering 13. Kt to Ksq by 13. . . P to KKt4, a plan from which he imprudently diverges.

(*d*) This excellent move prevents P to KKt4, for White would get in with the Knight to B5, gaining time by attacking a Pawn.

(*e*) 16. . . P×P was certainly a lesser evil. He could then play the Knight to K2.

(*f*) Obviously 17. . . . B×B; 18. P×Kt, P×P; 19. Kt to B5 would be fatal to Black.

(*g*) Best, for it secures possession of KB5 for his Knight.

(*h*) 20. Kt×P is wisely rejected. Black recovers the Pawn by B to B4 followed by Kt to K4, or even simply by Kt to K4. The King's move accordingly is not much use. Perhaps he feared 20. Kt to B5, B to B4, threatening Kt×P: but that could be guarded against when the necessity arises. Black now gains time for ingeniously defending the Pawn twice by bringing his Knight to a better square.

(*i*) In view of the coming attack it may seem better to play K to R2 at once, but the King would be strangely but promptly sent back by 24. Kt to K5! Better to have played P to B4 last move, and followed it by K to R2 and P to Kt3; for Black could have then answered Kt to K5 by Q to B2.

(*j*) Preventing Kt to K5 as in the last note, and preparing to sacrifice the exchange if necessary. See also note (*k*). The defence is most ingenious, and the game now becomes full of surprises.

(*k*) To defend the advanced Knight: for otherwise he cannot play P to Kt5 on account of ultimately R×Kt. It seems doubtful whether it would not be better to move the Queen to B2, attacking a Pawn at the same time, even if it is not quite safe to take it.

(*l*) A highly ingenious resource, which makes the game very difficult for both sides.

(*m*) Over-estimating his advantage. This exchange merely brings the other Knight (which has no move) into play, and loses the command of the point at K3. R to Kt4 looks good.

(*n*) A very clever trap, into which Mr. Mason unnecessarily, though probably intentionally, falls.

(*o*) 33. Kt×P is probably quite as good.

(*p*) Best. If 35. . . . Q×P, 36. Q to Ksq. wins.

(*q*) A mistake, which at once loses a hard-fought game. R to Ktsq was essential (not 38. . . . P to B3, 39. Q to Kt3 and wins). Even after 38. . . . R to Ktsq, 39. P to B6 must win shortly.

(*r*) There is nothing to be done. If 39. . . . R to KKtsq, 40. R×R, K×R; 41. Q to Kt3ch, K to Bsq; 42. Q to Kt7ch, K to Ksq; 43. Q to Kt8ch, K to Q2; 44. Q to Q8ch, etc.

CHESS INTELLIGENCE.

The International Tournament at Dresden resulted as follows:—

1st Prize	. .	Dr. Tarrasch (Nuremberg).
2nd } Prizes	. .	Makovetz (Austria).
3rd } Prizes		Porges (Hungary).
4th } Prizes	. .	Marco (Vienna).
5th } Prizes		Walbrodt (Berlin).
6th } Prizes	. .	von Bardeleben (Berlin).
7th } Prizes		S. Winawer (Warsaw).

The failure of the English contingent is most noticeable, though Mr. Blackburne secured the special prize given for the best score made against the prize winners. Mr. Mason was also for once unplaced, while Mr. Loman was absolutely last. This is Dr. Tarrasch's third consecutive victory in International Tournaments, in which, out of 52 games, he has lost only one—this was to Albin at Dresden, the result of inferior play in the opening.

The winners of the other chief prizes are comparatively unknown in England. Makovetz is perhaps the most talented of the four, Walbrodt being the most difficult to beat. The veteran Winawer made a fine start, but failed to sustain his form.

The Brighton meeting of the Counties Chess Association resulted, as last year, in a victory for Mr. J. H. Blake, of Southampton, who scored 6½ out of a possible 8. Mr. W. V. Wilson, of Brighton, was a good second with a score of 6. Mr. H. W. Butler, also of Brighton, was third; Messrs. Thorold and Trenchard dividing the fourth prize. The remaining players in order were E. Lambert (Exeter), A. Guest (London), A. Rumboll (Bath), and the Rev. A. B. Skipworth, who failed to score. Mr. Skipworth nearly won last year, and must have been handicapped by his duties as Hon. Sec.

The seventh volume of Mr. Morgan's Chess Library consists of a complete collection of all the 46 games played in matches and tournaments between Steinitz and Tschigorin. There are numerous diagrams, and a useful index of the openings, but no notes. The two telegraphic games are included. The price is one shilling.

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